EFFECTS OF FINANCIAL AND NON-FINANCIAL INCENTIVES ON RISKY HEALTH BEHAVIORS AND HEALTH OUTCOMES

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This dissertation is dedicated to all my teachers.

To my mom and dad, my very first teachers who encouraged me to dream big, made sacrifices for my education, and showed me by example the importance of serving others.

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Aparna Soni

EFFECTS OF FINANCIAL AND NON-FINANCIAL INCENTIVES ON RISKY HEALTH BEHAVIORS AND HEALTH OUTCOMES

This dissertation studies how individuals respond to the incentives in policies that aim to improve health outcomes and reduce risky behaviors. My research design exploits variation in individuals' out-of-pocket (OOP) medical prices generated by large insurance expansions. In Chapter 1, I study the effect of prices on the utilization of opioids and other prescription painkillers. I find that new users have a relatively high price elasticity of demand for prescription opioids, and that consumers treat over-the-counter painkillers as substitutes for prescription painkillers. My results suggest that increasing OOP opioid prices, through formulary design or taxes, may reduce new opioid use.

Chapter 2 examines whether increased access to pharmaceuticals improves elderly people's functional outcomes and reduces their dependence on long-term care. I exploit the introduction of Medicare Part D, which reduced OOP drug prices and expanded drug utilization among the elderly. I find that the policy increased seniors' capacity to perform activities of daily living and reduced the amount of time spent on informal caregiving by non-elderly caregivers.

Chapter 3 explores unintended effects of policies that expand prescription drug coverage. Economic theory predicts that lowering people's OOP health care costs may protect them financially from the consequences of their unhealthy behaviors. I use detailed data on individuals' food consumption and find that drug coverage worsens people's diets.

In Chapter 4, I exploit the Affordable Care Act (ACA) dependent coverage provision to assess the impacts of health insurance on consumption among young adults. I find that expanded insurance eligibility increased total spending, particularly in the categories of food, alcohol, and

contraceptives. I provide evidence that increases in consumer purchasing power may be an important spillover effect of health insurance expansions.

Chapter 5 analyzes the effects of the Medicaid expansions facilitated by the ACA on racial and ethnic disparities in cancer outcomes. We find that the Medicaid expansion had no detectable effect on cancer screenings for the overall population or for any specific race, but that the incidence of early stage diagnoses increased for Whites and by Hispanics; there was no detectable change for Blacks or other non-Hispanic races.

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Introduction

Certain modifiable health behaviors – such as tobacco and drug use, risky drinking, and diet – are important determinants of health, particularly in the United States and other industrialized countries, where mortality is primarily driven by chronic illness rather than infectious disease. An Institute of Medicine report shows that 48 percent of premature deaths in the US are linked to behavioral and other preventable causes (Institute of Medicine, 2015). In addition to increasing mortality, risky behaviors are also associated with higher morbidity: for example, obesity is correlated with arthritis and Type II diabetes, and smoking is associated with lung cancer (Cawley & Ruhm, 2011).

Traditional economic theory assumes that individual agents are rational, take into account all available information, and trade off the utility from a risky behavior against the costs of future adverse health outcomes. Under specific assumptions, these decisions are optimal and maximize individuals' net utility. In traditional economics, there is therefore little justification for government intervention in decreasing risky behaviors. However, behavioral economics posits that in reality, individuals exhibit bounded rationality, i.e. they have limited time and cognitive ability to make the best possible choices. Rather than maximize utility based on full consideration of all available information, they use heuristics, or rules of thumb, to make

decisions, and may be vulnerable to manipulation (H. Simon, 1955). Moreover, many risky behaviors are associated with addiction, and when individuals make decisions about current consumption, they may not take into account the expected effects that tolerance and withdrawal have on future utility (Cawley & Ruhm, 2011). Under the assumptions of behavioral economics, there could be a need for government to assist individuals in making rational decisions.

Even if individuals are able to make independent rational decisions about optimal levels of risky behaviors, there may still be a role for government intervention because individuals do not bear all the costs of their decisions; some of the costs are borne by other individuals (e.g. secondhand cigarette smoke and drunk driving accidents) or by society at large (e.g. public health insurers, criminal justice, and lost productivity). Studies find that social costs of risky health behaviors are high – $289 billion per year for smoking (US Department of Health and Human Services, 2014b), $223 billion per year for alcohol abuse (Bouchery, Harwood, Sacks, Simon, & Brewer, 2011), $186 billion per year for obesity (Cawley & Meyerhoefer, 2012), and $78 billion per year for prescription opioid abuse (Florence, Zhou, Luo, & Xu, 2016). Since individuals do not take into account these external costs when deciding to engage in risky behaviors, government intervention may be needed to align private and social costs.

That the government should play some role in incentivizing healthy behaviors is largely a politically and socially accepted idea in the United States. For decades, federal and state governments have attempted to reduce risky behaviors by increasing consumers' prices (e.g. cigarette, alcohol, and soda taxes), removing information asymmetries (e.g. prescription drug monitoring programs for controlled substances), and increasing access to health care (e.g. through expansion of public health insurance programs).

In order to understand the efficacy of these and future policies intended to improve health outcomes and behaviors, it is essential to know how individuals respond to the incentives built in these policies. To that end, this dissertation studies how financial and non-financial incentives impact individuals' health behaviors and health outcomes. I leverage variation in individuals' out-of-pocket (OOP) medical prices generated by large health insurance expansions, such as the introduction of Medicare Part D in 2006 and the Affordable Care Act (ACA) in 2010-14. I study the impacts of these OOP price changes on behaviors – such as opioid utilization, food purchases, and cancer screenings – and health outcomes – such as functional limitations and cancer diagnosis.

Summary of Dissertation Findings

I find that individuals respond to price changes for certain health behaviors/products but not for others. In Chapter 1, I study the effect of prices on the utilization of opioids and other prescription painkillers. I conclude that new users have a relatively high price elasticity of demand for prescription opioids, and that increasing OOP opioid prices, through formulary design or taxes, may reduce new opioid use. However, in Chapter 5, we find that even after the ACA Medicaid expansion decreased the OOP price of cancer screenings, there was no detectable effect of these price changes on utilization of cancer screenings for the overall population or for any specific race.

I also study the effect of insurance expansions on health outcomes and consumption. Chapter 2 examines whether increased access to pharmaceuticals improves elderly people's ability to perform activities of daily living (ADL), such bathing, dressing, and eating. I find that Part D increased seniors' capacity to perform daily activities and reduced their dependence on informal caregiving. In Chapter 4, I exploit the ACA dependent coverage provision to assess the

impacts of health insurance on consumption by young adults. I find that expanded insurance eligibility increased total spending, particularly in the categories of food, alcohol, and contraceptives. I provide evidence that increases in consumer purchasing power may be an important spillover effect of health insurance expansions.

Policymakers should also be aware of the unintended negative effects of policies that expand coverage. Economic theory predicts that lowering people's OOP medical costs may protect them financially from the consequences of their unhealthy behaviors. In Chapter 3, I use detailed data on individuals' grocery purchases and find that expanded pharmaceutical access associated with Part D worsened people's diets. These findings are informative about the presence of ex-ante moral hazard in prescription drug insurance as well as substitutability between certain prescription drugs and diet.

1 Health Insurance, Price Changes, and the Demand for Pain Relief Drugs: Evidence from Medicare Part D

Abstract

Overdose deaths from prescription opioids are on the rise, and policymakers seek solutions to curb opioid misuse. Recent proposals call for price-based solutions, such as opioid taxes and removal of opioids from insurance formularies. However, there is limited evidence on how opioid consumption responds to price stimuli. This study addresses that gap by estimating the effects of prices on the utilization of opioids as well as other prescription painkillers. I use nationally representative individual-level data on prescription drug purchases to exploit the introduction of Medicare Part D in 2006 as an exogenous change in out-of-pocket drug prices. I find that new users have a relatively high price elasticity of demand for prescription opioids, and that consumers treat over-the-counter painkillers as substitutes for prescription painkillers. My results suggest that increasing out-of-pocket prices of opioids, through formulary design or taxes, may be effective in reducing new opioid use.

1.1 Introduction

Prescription opioid utilization has nearly doubled over the past 15 years, even as use of non-opioid and over-the-counter (OTC) painkillers fell.[1] Although the medical purpose of opioids is to treat pain, these drugs are frequently misused due to their addictive properties. Prescription opioid misuse has devastating public health consequences, including increased overdose deaths, emergency department utilization, drug diversion, and crime (Council of Economic Advisers, 2017). Opioid overdose deaths now exceed 42,000 per year, and prescription opioids are responsible for between 34 and 77 percent of these deaths.[2] Moreover, prescription opioids often serve as a bridge to illicit heroin and fentanyl; studies have found that 80 percent of heroin users reported using prescription opioids prior to heroin (Jones, 2013), and heroin dealers specifically target areas with higher rates of opioid prescribing (Quinones, 2015). Thus, curbing prescription opioid use and initiation is a top public health priority.[3] Recent proposals call for price-based policies to reduce opioid consumption. The goal of this paper is to predict potential implications of these policies by estimating the price elasticity of demand for prescription opioids and identifying the effects of price changes on opioid initiation.

Policymakers can influence consumers' out-of-pocket (OOP) opioid prices through two main levers. First, state governments can implement opioid taxes, which may be passed down to consumers in the form of higher list prices.[4] So far, 15 states have introduced bills that – if

[1] See Figure 1-1 and Figure 1-2.

[2] The Center for Disease Control and Prevention's (CDC) mortality data does not distinguish deaths from pharmaceutical fentanyl and illegally produced fentanyl, so the prescriptions deaths displayed in Panel A of Appendix Figure 1- 2**Error! Reference source not found.** may include deaths from both types of fentanyl. Panel B of Appendix Figure 1- 2 uses an alternative way to classify deaths: the "semisynthetic and natural opioids" and the "heroin" bars refer unambiguously to prescription and illicit opioids, respectively. The "synthetic opioids" bar consists of deaths from both prescription and illicit fentanyl.

[3] See Appendix 1-A for additional details on the opioid crisis and policy efforts to curb opioid abuse.

[4] In general, prescription drugs are exempt from sales tax in all states, except Illinois (where they are taxed at 1 percent at the state level but exempt from local sales tax) and Louisiana – where they are tax-exempt at the state level, but local areas can opt to tax. In contrast, over-the-counter (OTC) drugs are subject to sales tax in all states

passed – would levy taxes or fees on prescription painkillers (Potter & Mulvihill, 2018).[5] Second, public insurers can revise their formularies to reduce coverage of the drugs, thereby increasing the portion of drug spending borne by consumers. For example, as of 2019, all Medicare Part D plans will reduce coverage of opioids for acute pain for opioid-naïve patients to 7 days. The current average length of a prescription is otherwise 22 days. Over 50 percent of opioid spending is from public sources (Appendix Figure 1- 1), so formulary changes in Medicare and Medicaid will likely have substantial effects. Even private insurance companies are taking steps to reduce inappropriate opioid utilization: several large insurers now impose similar 7-day limits for opioid-naïve patients, and the insurance giant Cigna ended coverage of Oxycontin in 2018.

These policies share the common goal of reducing equilibrium quantity of prescription opioids by increasing consumers' OOP prices. However, the effects of these policies depend on the price elasticity of demand for opioids. In spite of the prominence of pain relief drugs, little is known about patients' price sensitivity and the extent to which individuals substitute between addictive and less addictive painkillers. While an extensive literature documents a negative price elasticity of demand for prescription drugs in general (Coulson & Stuart, 1995; Duggan & Scott Morton, 2010; Gaynor, Li, & Vogt, 2007; Joyce, Escarce, Solomon, & Goldman, 2002; Ketcham & Simon, 2008; Lichtenberg & Sun, 2007; Yin et al., 2008),[6] these earlier findings may not apply to opioids because the impact of prices on drug utilization depends on the therapeutic class of drug (Gatwood et al., 2014; Goldman et al., 2004). Because opioids are addictive, it is

except Connecticut, Maryland, Minnesota, New Jersey, New York, Pennsylvania, Texas, Vermont, Virginia, and Washington DC. In Illinois, OTC drugs are taxed at a lower rate than other goods.

[5] In 2018, Kentucky voted on an opioid tax which would have levied a 25-cent on drug distributors for each dose sent to the state. Although the bill eventually failed to pass in the state Senate, the House did vote in favor of the tax, which suggests that there was considerable legislative support for the measure.

[6] Appendix 1-B provides a detailed review of the literature on price elasticities for prescription drugs.

plausible that opioid demand is less price elastic and that price elasticities are heterogeneous across new and existing users (Becker & Murphy, 1988).

The empirical challenge to obtaining unbiased elasticity estimates is to identify exogenous variation in drug prices. I accomplish this by exploiting shocks to OOP prices produced by the introduction of Medicare Part D in 2006.[7] My analysis distinguishes between opioids, which have a high risk for addiction, and non-opioid prescription painkillers (primarily NSAIDs), which carry relatively lower risks. I find that while the demand for non-opioid painkillers is not responsive to price changes, the price elasticity for prescription opioids is -0.9. This implies that consumers are more sensitive to the price of opioids than they are to other prescription drugs; previous studies that exploit Part D find price elasticity estimates of all prescription drugs ranging from -0.2 to -0.5 (Duggan & Scott Morton, 2010; Ketcham & Simon, 2008; Liu et al., 2011; Yin et al., 2008).[8] By providing some of the first evidence of the impact of OOP prices on consumers' demand for prescription opioids and other pain relief drugs, this paper contributes to the growing literature on price elasticities of prescription drugs.

Individuals may not be homogeneous with respect to price sensitivity, so I separately study subpopulations of interest, such as new opioid users, people with joint and back pain, cancer patients, and those with a history of drug poisoning. Policymakers wish to reduce the flow of new initiates because opioid-naïve patients who are prescribed opioids for acute pain relief are at high risk for developing new, persistent opioid abuse (J. S.-J. Lee et al., 2017; Shah, Hayes, & Martin, 2017). I find that the post-Part D change in opioid utilization came primarily from new

[7] For example, the OOP price of an opioid prescription for an elderly person fell from an average of $17 before Part D to $8 after Part D. For near-elderly individuals, in contrast, the OOP price changed from $15 to $11 over the same time period (author's calculations based on MEPS 2000-09).

[8] In Appendix Table 1- 4, I confirm the price elasticity of demand of all prescription drugs using a similar empirical approach as the approach used in the main analysis of this paper. I obtain an elasticity of -0.45, which is similar to that obtained in previous studies.

users who did not use opioids prior to 2006. On the other hand, there was no detectable effect of OOP prices for existing users. This finding contributes to the broader literature on how prices of addictive goods, such as cigarettes and alcohol, affect initiation (DeCicca, Kenkel, & Mathios, 2008; Saffer & Chaloupka, 1999). It is also important from a welfare perspective to understand potential responses among people with different types of medical conditions because public health experts view cancer and surgery as "legitimate" reasons to use opioids, whereas the use of opioids to manage joint and back pain is more controversial. If, for example, I find that cancer patients are the most price-sensitive group, then an opioid tax may be welfare-reducing.

Although there is some existing work on the demand for prescription opioids, little is known about the effects of prices on opioid initiation and heterogeneous consumption responses among people with different medical conditions. One previous paper uses Part D data to study the impact of entering the donut hole on the utilization of 150 different types of drugs; the authors estimate a small price elasticity of -0.04 for opioids (Einav, Finkelstein, & Polyakova, 2018). However, the study sample is limited to people who have spent up to the donut hole, i.e. those who are sicker and therefore more likely to be existing opioid users. In the Appendix of a working paper that studies the impact of Part D on drug diversion, the authors present evidence that Part D increased the number of opioid prescriptions by 28 percent and reduced OOP prices by 48 percent (implying a price elasticity of -0.6). However, this study does not address new versus existing users or other subpopulations of interest. The current paper makes important contributions by estimating how opioid-naïve people and existing users respond differently to price changes in opioids; I show that disregarding this distinction underestimates the full effect of price changes. I also identify price elasticities separately for people with different medical conditions, and show that price increases do not differentially affect cancer patients (who have

uncontroversial "legitimate" reasons for opioid use) and that those with back and joint pain (controversial justification for opioid use) are more likely to respond to price changes. Section 1.7 offers additional discussion of my results in light of the existing literature.

1.1.1 Substitution between Prescription and Over-the-Counter Painkillers

The second contribution of this paper is to estimate cross-price elasticities of demand between prescription painkillers and OTC painkillers. These estimates are important from a policy perspective because promoting substitution toward other effective but less addictive treatments for pain has been proposed as a way to address the opioid crisis (Centers for Disease Control and Prevention, 2016). OTC painkillers are substantially less addictive, are less costly for the government, and have fewer negative spillover effects such as drug diversion. However, there are few studies that study potential substitution between prescription and OTC drugs, and what little evidence exists is primarily based on observational rather than experimental data (Leibowitz, 1989; O'Brien, 1989; Stuart & Grana, 1995). Moreover, none of these existing studies specifically analyzes painkillers.

Part D is an appropriate setting to study potential substitution between prescription and OTC drugs. The elderly are heavy users of both types of drugs (Qato, Wilder, Schumm, Gillet, & Alexander, 2016), and the implementation of the policy lends itself to quasi-experimental analysis, which reduces concern about selection bias. I use scanner data on households' grocery and drug purchases to study the effect of the prescription OOP price reduction associated with Part D on people's OTC painkiller purchases. I estimate a small but positive cross-price elasticity of demand for OTC painkillers (elasticity = 0.1), which implies that consumers view prescriptions and OTC painkillers as substitutes to some extent. My findings suggest that a

targeted subsidy for OTC painkillers may be an effective way to shift demand away from opioids.

The remainder of this paper proceeds as follow. Section 1.2 proposes a conceptual framework for predicting the effects of prices on the demand for addictive painkillers and their substitutes. Section 1.3 presents the Medical Expenditure Panel Survey and Nielsen Household Consumer Panel datasets utilized in this analysis. Section 1.4 describes the empirical methods and results for the impact of price changes on utilization of prescription painkillers. Section 1.5 presents results for new versus existing users. Section 1.6 provides cross-price elasticity estimates for OTC painkillers with respect to prescription painkiller prices, and Section 1.7 concludes.

1.2 Conceptual Framework

In this section, I develop a general theoretical framework to predict how changes in OOP prices of prescription painkillers will affect quantity demanded.[9] I assume that the demand for pain relief is a derived demand for health (Grossman, 1972). Individuals maximize lifetime utility (U) – which is a function of total consumption of all goods (Y), pain relief (P), and addictive capital (S) – subject to a lifetime budget constraint. Pain relief itself depends on consumption of addictive prescription painkillers (opioids, or O), non-addictive prescription painkillers (NSAIDs, or N), and non-addictive OTC painkillers (C). Quantity demanded of each of the three types of painkillers depends on individuals' incomes as well as the portion of the drug price they are responsible for paying (i.e. OOP prices). By increasing prescription drug

[9] My data measures *utilization* of drugs, which may not be synonymous with demand. Utilization is based on patients' demand for the drug as well as physicians' willingness to write prescriptions. A reduction in OOP price can increase utilization in three ways: 1) encourage patients to seek prescriptions by increasing physician visits, 2) increase the number of prescriptions written by physicians, and 3) increase the number of prescriptions that are filled (compliance).

coverage in the elderly population, Part D resulted in an exogenous decrease in the OOP price of prescription painkillers, but did not affect the price of OTC painkillers as these drugs are not covered by insurance companies. I assume that all three types of painkillers are positively associated with utility (i.e. $U_O > 0$, $U_N > 0$, and $U_C > 0$).

Proposition 1. *Assuming conventional downward sloping demand curves, a reduction in the price of prescription opioids (non-opioid prescription painkillers) should increase quantity demanded of prescription opioids (non-opioid prescription painkillers), holding income and other prices constant.*

However, opioids are addictive goods and may not obey the law of demand: it is plausible that physiological forces associated with dependence and addiction may compel a person to continue consuming a good, even if economic incentives change. My model accounts for opioids' addictive properties by including addictive capital in the individual's utility function. Addictive capital is measured by the stock of total past consumption of the addictive painkiller. I assume that addictive goods (O) have the three characteristics described below (Cawley & Ruhm, 2011).

1. Withdrawal: Consumption of the addictive goods reduces symptoms associated with withdrawal, so the marginal utility of current consumption is positive ($U_O > 0$).
2. Tolerance: Being addicted has overall harmful health consequences, so the stock of past consumption lowers utility ($U_S < 0$).
3. Reinforcement: The marginal utility of current consumption rises with the stock of past consumption ($U_{OS} > 0$).

Proposition 2. *For addictive painkillers, new users are more price-sensitive because they have not yet built up enough addictive capital to make future prices and consumption a significant consideration in their decision-making.*

A large literature on consumer behavior in other markets with addiction finds that while existing users of addictive goods are less sensitive to price changes, prices do affect the

probability of initiation by new users. For example, one study finds that a 10 percent increase in the price of alcohol was found to decrease the probability that an individual currently drinks by 5.5 percent; the same study finds that the heaviest drinkers are least price sensitive (Manning, Blumberg, & Moulton, 1995). In the cigarette market also, studies show that price sensitivity varies by intensity of use. A meta-analysis shows that while the mean price elasticity of demand for cigarettes is -0.5, estimates vary widely ranging from -3.1 to 1.4 (Gallet & List, 2003). Specifically, higher cigarette prices can lead to large decreases in the probability of initiation by non-smokers (Gilleskie & Strumpf, 2005). The literature also finds that excise taxes on cigarettes can significantly deter smoking among adolescents, who have had less time to become addicted to the good as compared to older adults (Chaloupka & Wechsler, 1997; Gruber, 2001; Gruber & Zinman, 2000; Lewit, Coate, & Grossman, 1981). This inverse relationship between intensity of use and price elasticity exists in the market illicit drugs also. In the cocaine market, for example, the price elasticity of demand is -1.0 for the general population, but only -0.3 for those who are current users (Chaloupka, Grossman, & Tauras, 1999).

Proposition 3. *Existing users may also respond to price changes of the addictive good if they behave as rational addicts.*

The Theory of Rational Addiction proposes that consumers are sophisticated and account for tolerance and reinforcement when deciding current consumption (Becker & Murphy, 1988). Reinforcement implies that consumption of the addictive good today will positively affect the individual's marginal utility of consuming the addictive good tomorrow. This means that a price change in the addictive good may compel forward-looking addicts to change their consumption habits.

Proposition 4. *If OTC and prescription painkillers are substitutes, the quantity demanded of OTC painkillers falls when the price of prescription painkillers falls.*

The individual in my model maximizes pain relief by choosing an optimal mix of O, N, and C. The optimal mix depends on their relative prices and their relative productivities. Previous medical studies suggest that prescription and OTC painkillers are therapeutic substitutes for certain medical conditions (Chang, Bijur, Esses, Barnaby, & Baer, 2017). If consumers view prescription and OTC painkillers as economic substitutes, we should expect to see a reduction in C after Part D reduces the prices consumers face of O and N.

1.3 Data

This study uses two main data sources: the household component of the Medical Expenditure Panel Survey (MEPS, years 2000 to 2009) and the Nielsen Household Consumer Panel (NHCP, years 2004 to 2009).[10] The MEPS is a nationally representative survey that provides detailed information on individuals' medical expenditures, pharmaceutical utilization, and health outcomes (Agency for Healthcare Research and Quality, 2017). The MEPS is conducted annually, and the survey follows a panel design, featuring five rounds of interviews covering two full years. The original sample size is approximately 35,000 individuals per year; my analytical sample consists of 50,579 individuals aged 55 to 74 across the years 2000 to 2009. My analysis uses the MEPS full-year Consolidated Data File, which contains respondents' socio-demographic and economic characteristics; the MEPS Prescribed Medicines file, which contains

[10] In selecting the appropriate time period for this analysis, I note that including additional years of post-2006 data would increase the sample size but may also bias the results by introducing other notable events that should have differentially affected the elderly and near-elderly. For example, the Affordable Care Act of 2010 increased overall health insurance access for the near-elderly group but not for the elderly group (Frean et al., 2017). The Oxycontin reformulation and removal of Darvocet (e.g. Propoxyphene) also occurred in 2010 and significantly changed the landscape of the opioids market. I therefore limit my period of analysis to pre-2010 years.

all the prescription drugs purchased by respondents;[11] and the MEPS Medical Conditions File, which describes all medical conditions and treatment attempts.

The MEPS is uniquely suited for this study as it contains detailed information on prescription medication use in the years relevant for this study. Purchases of prescription drugs are reported by individual respondents and then verified by the prescribing pharmacy.[12] The MEPS provides comprehensive information on medication characteristics, including the drug name, form, strength, quantity purchased, and National Drug Code. Other datasets, such as the National Health Interview Survey and Behavioral Risk Factors Surveillance System, have the advantage of larger samples but do not contain prescription data. The Part D claims data does not have information on individuals before 2006. The MEPS has been used in past studies to study the effects of Part D on drug utilization (Alpert, 2016; Engelhardt & Gruber, 2011; Powell, Pacula, & Taylor, 2017). However, the MEPS has limitations, such as relatively small sample sizes. Also, any individual panel only contains two years of observations, which limits the ability to estimate long term effects of the policy change. Table 1-1 provides descriptive statistics of the MEPS sample.

Because the MEPS provides data only for prescription drugs and not for OTC drugs, I use the NHCP to obtain information on purchases of the latter (Nielsen, 2017). The NHCP contains detailed information on grocery and drugstore purchases of a panel of 40,000 to 60,000 households. My analytical sample consists of 335,060 household-year observations across the years 2004 to 2009. Variables include household demographics, geographic identifiers (to the zip code level), and product characteristics (to the UPC code level). I use the NHCP to acquire data

[11] The Prescribed Medicines file consists of only outpatient prescription drug purchases and excludes prescription drug administered in hospitals, clinics, or physician's offices.

[12] The data has been verified by the prescribing pharmacy only for those who consented to release their pharmacy records. For those who did not consent, expenditures are based on self-reported expenditures that have been adjusted for outliers and imputations from the pharmacy data.

on households' OTC drug costs and utilization. Table 1-2 provides descriptive statistics of the NHCP sample.

The two main outcomes of interest in this paper are quantity purchased of a drug class and OOP price. I first calculate the percent change in OOP prices caused by Part D and the percent change in quantity purchased of the drug class caused by Part D. Then using the following elasticity formula, I obtain the estimated price elasticity of demand for the drug class.

$$\varepsilon = \frac{Percent\ Change\ in\ Quantity}{Percent\ Change\ in\ Price}$$

Equation 1-1

In my main analysis, I measure quantity as the units of days supplied of the drug for each person-year observation; days supplied can range from 0 to 365. Although MEPS provides information on the quantity of drugs purchased, the unit varies depending on the type of drug. Painkiller prescriptions come in different forms, including immediate release tablets, extended release tablets, liquid solutions injections, and patches. The reported MEPS quantity may be in number of bottles, number of pills, number of ounces, number of patches, etc. To obtain a consistent unit, I convert all purchases to "number of days supplied" of the drug. For example, for a strong oxycodone, a 28-pill bottle might mean a 28-day supply, but for a mild NSAID, a 28-pill bottle might mean only a 7-day supply. A similar days supplied measure has been used in previous Part D studies (Ketcham & Simon, 2008; Lichtenberg & Sun, 2007; Yin et al., 2008). MEPS provides information on days supplied from the years 2010 onward, so for earlier years, I impute the number of days supplied of each drug using post-2010 data of the same drug.[13] I also conduct sensitivity analyses in which the quantity is measured as number of prescriptions, rather than "number of days supplied."

[13] See Appendix 1-C for additional details on the imputation process.

To measure price, I use the OOP price (adjusted by pharmaceutical PPI) as my key outcome variable since this is the price faced by the individual. MEPS provides information on the total price paid for each prescription, as well as the breakdown by source of payment. For the NHCP outcomes, I simply use the reported price as my outcome, since these drugs are all purchased over the counter so other payment sources do not exist.

I first estimate elasticities for all painkillers combined. However, painkillers vary widely in terms of both strength and potential for abuse. I therefore categorize the drugs into two classes, based on their risk for addiction and dependence:

1. Opioids: Pain relief drugs whose distribution is controlled by the US Drug Enforcement Administration (DEA) because they have potential for abuse and can lead to physical or psychological dependence. This class includes drugs such as codeine, fentanyl, hydrocodone, oxycodone, tramadol, and opioid combinations such as hydrocodone and acetaminophen.
2. Non-Opioid Painkillers: Pain relief drugs that must still be obtained via a prescription but are not controlled by the DEA because they have no known potential for abuse. These are mostly prescription-strength NSAIDs, such as Aspirin and Ibuprofen, and Acetaminophen.[14]

Table 1-3 provides additional details about the composition of each class.[15] In addition to analyzing these three broad classes of painkillers (all painkillers, opioids, and non-opioid painkillers), I separately assess the opioids category by:

1. High-dose vs. low-dose opioids: I define high-dose opioids as prescriptions that contain greater than 90 morphine milligram equivalents (MME) per day. In the MEPS, the mean (median) MME per day for an opioid prescription is 43 (30). I obtain information on MME from the CDC website (National Center for Injury Prevention and Control, 2017). I also examine total MME consumption as a continuous outcome variable.
2. Extended-release vs immediate-release opioids. Extended-release formulations, such as Oxycontin, are designed to release slowly into the bloodstream and have the advantage of being taken at less frequent intervals than their immediate-release counterparts.

[14] Although NSAIDs have no known potential for addiction, they are not without risk. Side-effects of prolonged NSAID use include liver damage and GI bleeding. Nevertheless, most studies find that opioids represent a substantially higher risk of death and adverse events than NSAIDs (Solomon, 2010).

[15] Table 1-3 provides an abridged version of the painkiller classification. See Appendix Table 1- 2 for the complete classification.

In supplementary analysis, I also analyze changes in the consumption of the most commonly used opioids by the elderly during 2000-09: hydrocodone, propoxyphene, oxycodone, tramadol, codeine, morphine, fentanyl, and methadone. Such analysis is useful because even within the opioids class, different drugs may pose different public health risks. For example, the drugs most often involved in prescription opioid overdose deaths are oxycodone, hydrocodone, and methadone. The most frequently diverted drugs are oxycodone and hydrocodone.

1.4 Impact of Price Changes on Prescription Painkiller Utilization

1.4.1 Empirical Methods

The empirical objective of this study is to estimate the effect of OOP drug prices on utilization of prescription painkillers. A naïve approach to this question might examine the cross-sectional relationship between observed drug prices and purchases. However, even with a rich set of control variables, this approach would not identify the causal effect of price on utilization because of the likely presence of latent confounds; it is not possible to calculate an unbiased estimate unless we know whether price changes are due to a supply shock or a demand shock. A reasonable alternative method may be to use prescription drug coverage as an instrument for price, as there is substantial empirical evidence to show that obtaining drug insurance lowers the OOP price of drugs. However, simply comparing drug uninsured with drug insured individuals would not yield an unbiased causal estimate because of selection: people who are in worse health are more likely to enroll in generous insurance plans as well as consume more drugs; this would bias the estimate upwards.

In order to overcome this endogeneity, I propose a difference-in-differences (DD) estimation strategy that exploits the introduction of Medicare Part D[16] in January 2006 as an exogenous change in OOP drug prices for a treatment group of Medicare enrollees.[17] Part D provided publicly subsidized prescription drug coverage to Medicare eligibles and reduced the fraction of drug-uninsured elderly from 26 percent to 8 percent in its first year (Appendix Figure 1- 3). Thus, the policy represented a sharp decrease in OOP drug prices for many people over the age of 65 who previously lacked drug coverage, while it was less likely to affect prices for younger people who were ineligible for the policy.[18] In contrast to insurance plans that individuals select and fully pay for themselves, Part D plans were available at highly subsidized rates to all Medicare-eligible adults and are therefore less likely to correlate with other factors that affect the demand for drugs. Part D has been used extensively to study causal effects of prescription drug coverage (Basu, Yin, & Alexander, 2010; Duggan & Scott Morton, 2010, 2011; Engelhardt & Gruber, 2011; Ketcham & Simon, 2008; Lichtenberg & Sun, 2007; Yin et al., 2008).[19]

I estimate difference-in-differences (DD) models, comparing utilization among a treatment group that was affected by Part D (those aged 65 to 74, N=22,265) with those who

[16] See Appendix 1-D for additional background on Medicare Part D.

[17] Other researchers have used the RAND Health Insurance Experiment (HIE) to estimate the price elasticity of demand for health care overall (Manning, Newhouse, Duan, Keeler, & Leibowitz, 1987). While the HIE is useful in identifying the effects of cost sharing for most medical services, plan design did not differ independently for drug coverage, making it difficult to isolate the impact of drug price changes. Moreover, the HIE data is from the 1980s, whereas prescription painkillers became more popular in the late 1990s; consumer preferences for painkillers were likely very different in the 1980s than in more recent years.

[18] Although 74 percent of the elderly had prescription drug coverage even before 2006, this coverage was often less than adequate. We may expect drug utilization to increase even for those who had coverage before 2006 if Par D coverage was more generous than previous drug plans, e.g. offered lower cost-sharing, fewer restrictions such as prior authorization, or more medications covered in formularies.

[19] While Part D is an older policy, it is still a topic of discussion in the current literature because provides a valuable context for studying the causal effects of increased pharmaceutical access (Bradford & Bradford, 2016; Buchmueller & Carey, 2018; Carey, 2017; Dunn & Shapiro, 2019; Huh & Reif, 2017; Kaplan & Zhang, 2017; Powell et al., 2017). The purpose of the current analysis is not to evaluate the impact of Part D as a policy, but rather to understand more generally how utilization of prescription painkillers responds to prices; Part D merely serves an identification strategy.

were not affected (those aged 55 to 64 and not on Medicare, N=28,314),[20] before and after the introduction of the policy in January 2006. The use of the near-elderly control group helps separate Part D's effects from other secular factors that may have changed at the same time (e.g., drugs going off patent). Specifically, I treat the MEPS data as a series of repeated cross sections and estimate the following baseline model for each utilization outcome described in Section 1.3:

$$Y_i = \alpha + \beta(Treatment_i \times Post_i) + \gamma X_i + \sum \eta_j Age_i + \vartheta_i + \varepsilon_i$$

Equation 1-2

where Y_i represents the number of days supplied of a drug for individual i, $Treatment_i$ is an indicator equal to one if the individual belongs to the treatment group, $Post_i$ is an indicator equal to one for observations following January 2006, X_i is a vector of demographic control variables (sex, marital status, household income, educational attainment, race/ethnicity, and region), Age_i is a vector of age-fixed effects, ϑ_i is an indicator variable for each year, and ε_i is an idiosyncratic error term. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. The DD coefficient of interest β represents the change in drug utilization for elderly individuals following the introduction of Part D, relative to the change for near-elderly individuals.

I use Equation 1-2 to estimate the effect of Part D on utilization outcomes, but not on OOP price outcomes. This is because OOP prices in MEPS are observed only for individuals who actually buy the drugs, and changes in observed OOP prices may be driven by three phenomena: 1) list prices of drugs decreased after Part D due to insurers' increased bargaining power (Duggan & Scott Morton, 2010); 2) expanded drug coverage reduced OOP price faced by consumers; and 3) consumers likely responded to increased drug coverage by substituting to

[20] While this classification of treatment and control groups works in the MEPS, the NHCP is a household-level dataset, and households can consist of individuals of differing ages. Nevertheless, the NHCP provides detailed ages of each household member, so I define the treatment group as households with at least one member aged 65-74, and the control group as households with all members <65.

more expensive drugs which would seemingly increase the average OOP prices observed in the data. Equation 1-2 would capture both the static effect of declining list and OOP prices (holding constant the pre-Part D mix of drugs), as well as the dynamic effect of elderly individuals shifting consumption to more expensive drugs in response to increased drug coverage. However, the denominator of Equation 1-1 should ideally represent only the static effect. I isolate the static effect by identifying the pre-Part D "basket" of painkillers purchased by the elderly and the near-elderly pre-Part D and using a different DD model to estimate Part D's effect on changes in OOP price for this fixed basket of drugs.

For this analysis, I create an NDC-treatment group-year dataset. I first calculate the number of days supplied of each NDC in the year 2003 separately for the treatment group and the control group (adjusting for MEPS survey weights).[21] Appendix Table 1- 3 presents the composition of the 2003 basket of pain relief drugs for each group. I then calculate the average OOP price per day supplied for each NDC-treatment group-year observation (adjusting for MEPS survey weights). Next, I estimate the following equation:

$$Y_{dgt} = \alpha + \beta\left(Treatment_g \times Post_t\right) + \mu\left(Treatment_g\right) + \vartheta_t + \varepsilon_{dgt}$$

Equation 1-3

where Y_{dgt} represents the out-of-pocket price per day supplied of NDC *d* purchased by treatment group g in the year *t* and other variables are defined as in Equation 1-2. Importantly, the regressions are weighted by the 2003 level of utilization. The DD coefficient of interest β represents the change in OOP price for elderly individuals following the introduction of Part D, compared to the change for near-elderly individuals. A similar approach was used for estimating drug elasticities in previous studies (Chandra, Gruber, & McKnight, 2010; Contoyannis, Hurley, Grootendorst, Jeon, & Tamblyn, 2005; Landsman, Yu, Liu, Teutsch, & Berger, 2005)

[21] Part D was signed into law at the end of 2003. The year 2003 is therefore unlikely to be biased by possible anticipation effects (Alpert, 2016).

1.4.2 Caveats

There are four primary concerns with this identification strategy. First, Part D simultaneously changed seniors' OOP prices for all drugs. To the extent that consumers consider opioids and non-opioid painkillers substitutes, my elasticity estimates may be biased downward. (If there were to be a reduction in the OOP price of opioids only and no change in the OOP price of non-opioid painkillers, we would expect a larger utilization response than in the case where OOP prices of both classes reduced simultaneously. My elasticity estimates can therefore be interpreted as a lower bound.) This is a common issue in existing studies that estimate drug-specific elasticities, since policy-induced price variation is usually not drug-specific (Chandra et al., 2010; Einav et al., 2018; Goldman et al., 2004).

Second, Medicare Part D was signed into law in late 2003 but not implemented until January 2006. Elderly individuals in 2004-05 may have delayed drug purchases in anticipation of gaining Part D coverage in 2006 (Alpert, 2016). Alternatively, in post-Part D years, those who are near the age of 65 may delay drug purchases until they gain Part D coverage after age 65. This possibility, if it exists, would bias my estimates downward and may increase the likelihood of Type II error. I account for this possibility by estimating a set of DD models in which I split the "post" period into two time periods: 2004-05 and 2006-09. I also estimate specifications of Equation 1-2 that omit the years 2004 and 2005 from analysis and omit 63- and 64-year-olds from the sample.

Perhaps Part D influenced opioid purchases through non-price mechanisms, e.g. if the policy increased pharmaceutical advertising and detailing in a way that made elderly individuals more likely to seek out opioids and instigated physicians to prescribe more opioids. While this is theoretically plausible, empirical studies have found limited evidence that Part D influenced

advertising of opioids. An analysis of the effects of direct-to-consumer advertising found that although Part D increased pharmaceutical advertising, opioids are among the top 10 *non*-advertised drug classes for older adults (Alpert, Lakdawalla, & Sood, 2015). Moreover, the authors find little evidence that Part D caused changes in physician detailing.

Finally, my sample includes only those aged 55 to 74, so there may be concerns about extending my conclusions about price sensitivity to those outside this age group. In spite of these concerns about external validity, the elderly are an important group to study because they are the largest users of prescription opioids. The majority of prescription opioid growth over the past 15 years came from those aged 65 and older (100 percent increase in prescription opioid utilization over this time period) and those aged 45 to 64 (71 percent increase in utilization). Conversely, adults aged 18 to 44 and children younger than 18 saw only marginal changes in their prescription opioid utilization (Panel B of Figure 1-1). Moreover, Medicare is the largest payer of opioid pain relievers, covering 20 to 30 percent of opioid spending since 2006 (Zhou, Florence, & Dowell, 2016), another indication that it is important for federal policymakers to understand how this population responds to price stimuli.

1.4.3 Baseline Results

Table 1-4 displays both pre-2006 means for the treatment group and DD estimates from Equation 1-2 and Equation 1-3 for the impact of Part D on painkiller utilization (Columns 1-3) and OOP price (Columns 4-6). Column 5 displays the implied elasticity estimate (calculated using the results from the first six columns). The first row of Table 1-4 shows that Part D led to a 4.3 increase in the number of days supplied of all prescription painkillers ($p<0.10$), which represents an 11 percent increase compared to pre-2006 levels. Part D also led to a $0.51 decrease in the OOP price per day supplied of all prescription painkillers ($p<0.01$), which

represents a 38 percent decline from pre-2006. The implied elasticity is therefore -0.29 (calculated by dividing 11 percent by -38 percent). This result suggests that the demand for prescription painkillers is downward sloping and slightly inelastic; a 10 percent decrease in price would lead to a 2.9 percent decrease in quantity demanded.

However, when I stratify the painkillers into opioids and non-opioids, I find that the elasticities vary widely. While there is no detectable effect of OOP prices on the demand for non-opioid painkillers, the demand for opioids is more elastic (ε=-0.89). Subsequent panels of Table 1-4 show that the majority of the increase in opioids purchases came from low-dose opioids and extended-release opioids. It is also interesting to note that Part D led to a large increase of 74 percent in the total MMEs consumed. Most of the increase in opioid utilization came from hydrocodone and morphine (Appendix Table 1- 5).

1.4.4 Parallel Trends Tests

The key identifying assumption of the DD model is that in the absence of Part D, both groups would have trended similarly. One way to evaluate the plausibility of this assumption is to compare descriptive statistics from the two groups. Table 1-1 reports statistics for MEPS respondents in the treatment and control groups just prior to Part D's implementation. Individuals in the treatment group are significantly less likely to be married (plausibly because people in the treatment group are older and more likely to be widowed), less educated, and have lower household income (likely because more people in the treatment group are retired) than those in the control group. However, the treatment and control group do not differ substantially in gender composition, race/ethnicity, and region of residence.

More important than comparing descriptive statistics is to assess whether the two groups exhibit comparable pre-2006 trends in their OOP painkiller prices and utilization. Figure 1-4

presents the average OOP drug prices for each NDC over time, weighted by the 2003 level of utilization. Figure 1-4 shows that prior to the introduction of Part D, OOP prices for the treatment and control group followed similar trends. After 2006, both groups experienced declines in OOP prices, but the decline for the treatment group was much larger in magnitude than the reduction experienced by the control group. The fact that prices for the two age groups trended similarly before 2006 increases our confidence that they would have trended similarly after 2006, were it not for the introduction of Part D.

Similarly, Figure 1-5 presents the utilization for each class of painkillers over time, separately for the treatment and control group. Again, purchases of painkillers appear to trend fairly closely for the older and younger groups in the years before Part D. There was a sizeable reduction in non-opioid painkiller utilization for both groups in 2004-05; this decrease can be attributed to the removal of certain widely used Cox-II inhibitors (e.g. Vioxx, Bextra, etc) from the market in late 2004 and early 2005. After the implementation of Part D in 2006, there was a large increase for the treatment group, while the control group's utilization remained constant or trended upward more gradually.

To formalize the relationship illustrated in Figure 1-4 and Figure 1-5, I estimate a specification of Equation 1-2 that replaces the $Treatment_i \times Post_i$ term with a series of interaction terms between the treatment group indicator and an indicator for each year. I omit the year 2005 as the reference year. Specifically, I estimate the following equation:

$$Y_i = \alpha + \sum \beta_j \, (Treatment_i \times Year_i) + \gamma X_i + \sum \eta_j Age_i + \vartheta_i + \varepsilon_i$$

Equation 1-4

where $Treatment_i \times Year_i$ represents the interaction between the treatment indicator and the year indicator for each year except 2005. All other variables are defined as in Equation 1-2. Table 1-5 presents the coefficient estimates of the β_j terms for the utilization and OOP price

outcomes for all painkillers, opioids, and non-opioid painkillers. For all the main outcomes presented in Table 1-5, the pre-2006 β_j terms are statistically indistinguishable from 0. I also conduct an F test of whether the point estimates for all the pre-2006 β_j terms are jointly different from zero. For all outcomes, I cannot reject the null hypothesis at a p-value of 0.10. Appendix Table 1- 6 presents results for the remaining outcomes. Of the 10 outcomes presented in Appendix Table 1- 6, I reject the null hypothesis of parallel trends for only one outcome – utilization of extended-release opioids. Together, the evidence suggests that the near-elderly control group services as a reasonable comparison group for the utilization responses of the elderly treatment group.

Table 1-5 also shows that the larger utilization effects came in 2007 and later. This finding is consistent with previous studies that find substantial impacts of Part D on utilization only after the second half of 2006 (Yin et al., 2008). This is likely because enrollment of seniors into Part D was gradual during the first half of 2006; earlier Part D enrollees were sicker and less likely to respond immediately to price changes.

1.4.5 Heterogeneity Tests

In Table 1-6, I use respondents' reported conditions to assess the effects of Part D on prescription painkiller utilization for subpopulations that are of interest to policymakers. In the first panel, I stratify the sample into individuals who have cancer and those who do not. Opioids are widely accepted as legitimate pain treatment for cancer patients (Centers for Disease Control and Prevention, 2016). I find that the OOP price reductions associated with Part D led to a 45 percent increase in opioid utilization for people with cancer and 49 percent increase for those without cancer. Part D led to a 37 percent increase in opioid utilization for those with joint or

back pain and had no detectable effect for those without joint or back pain. Finally, I stratify the sample by whether respondents had medical or non-medical substance poisoning. I find that those who had a poisoning event did not experience any significant change in painkiller utilization after Part D, whereas those who did not have a poisoning event increased opioid utilization by 52 percent when OOP prices dropped.

1.4.6 Sensitivity Analyses and Robustness Checks

Despite the parallel trend test regarding the comparability of individuals in the elderly and near-elderly groups, there may be lingering concerns about the parallel trends assumption. To provide additional confidence in the causal interpretation of β, I conduct falsification tests which estimate a series of models similar to Equation 1-2, but define $Treatment_g$ as different 10-year age groups whose eligibility for drug coverage was unaffected by Medicare Part D: non-disabled individuals aged 45-54, 35-44, 25-34, and 18-24. I expect to find no effect of Part D on prices and utilization for these "false" treatment groups relative to the control group of those aged 55-64. If I do find significant effects, it would imply that the model is biased due to violations in the parallel trends assumption. Failure to find significant effects will provide additional confidence in the approach. Results for these falsification tests are presented in Appendix Table 1- 7. Of the 12 falsification tests, I reject the null hypothesis at a significance level of 0.10 for only one outcome – utilization of opioids for the false treatment group consisting of individuals aged 18-24. However, the coefficient is in the opposite direction as expected, i.e. utilization of opioids for those aged 18-24 decreased relative to the 55-64 group.

Appendix Table 1- 8 displays results from a specification in which I split the post period into two periods to study potential anticipation effects from the announcement of Part D in late 2003. For all painkillers, there was a marginally significant increase in utilization even during

2004-05 ($p<0.10$). However, this rise was smaller in magnitude than the 2006-09 increase. For opioid painkillers, the increased utilization happened only in 2006-09; for non-opioid painkillers, there was no detectable effect in either time period. Results in the second and third column of Appendix Table 1- 9 provide additional confidence in the finding that potential anticipatory effects do not bias my results. The second column displays results from a specification of Equation 1-2 that omits the years 2004 and 2005 from analysis, and the third column displays results from a specification that omits 63- and 64-year-olds from the sample. In both cases, the results are very similar to the baseline estimates.

Next, I expose Equation 1-2 to a number of sensitivity analyses. Appendix Table 1- 9 presents these results. In the first column of Panel A, I omit the demographic control variables from the right hand side. In column 2, I omit the years 2004-05 from analysis. Column 3 omits respondents aged 63-64 from the control group. In the fourth column, I use an alternative definition of "treatment" in which I omit younger Medicare recipients (rather than include them in the treatment group). In column 5, I include a vector of interaction terms for the treatment group indicator with an indicator for each year on the right hand side. The sixth column includes a right-hand side variable that controls for the respondent's health status (measured by their total medical expenses in the year). In the seventh column, I include both treatment X year fixed effects and control for respondent's health status; this is to account for the fact that older individuals are in worse health than younger ones. Panel B presents results in which I include additional years of MEPS data in the analysis. All of these sensitivity analyses yield results that are remarkably similar to those presented in the baseline model.

In Appendix Table 1- 10, I conduct another sensitivity analysis in which I change the units of my outcome to "number of prescriptions" of the drug rather than "number of days

supplied." I do this because my original outcome variables involved an imputation to convert prescriptions into days supplied. Although the magnitudes of the estimates are expectedly different because of the different unit used, qualitatively the outcomes are very similar to the original specification.

My baseline analysis studies the aggregate effects of Part D on painkiller utilization. The DD estimate captures the direct effect experienced by those who were previously drug uninsured and gained prescription drug coverage through Part D, as well as substitution effects for those who switched from private drug insurance to Part D once the publicly subsidized option became available. While many studies in the Part D literature use this DD approach to study aggregate effects, it is important to note that 74 percent of the elderly had drug coverage even before the introduction of Part D. Thus, the DD estimate may underestimate the true effect of gaining new drug coverage. To provide suggestive evidence, I hone in on income groups that were particularly likely to gain new coverage to Part D and find that the largest effects on utilization came from middle-income individuals with household income between 125 and 400 percent of the poverty level (Appendix Table 1- 11). This is consistent with previous studies that find that middle-income individuals were more likely to gain drug coverage after 2006, since low-income elderly people likely had drug coverage through Medicare and high-income people likely had coverage through employer insurance (Levy & Weir, 2009).

My sample includes people who are aged 55 to 74 (i.e. plus and minus 10 years from the age 65 cutoff). In Appendix Table 1- 12, I assess whether my results are sensitive to the selection of age groups included. I do this by first estimating Equation 1-2 for a sample with only people aged 50 to 79 (with people below 65 defined as the control group and people 65 and over defined as the treatment group). I then restrict my sample to people aged 51 to 78, then 52 to 77, and so

on until I reach people aged 60 to 69. For each sample, I obtain results that are remarkably similar to my original set of results that use people aged 55 to 74. This suggests that the results are not sensitive to the selection of age groups included in the sample.

1.5 Elasticity Estimates for New vs. Existing Users

The results discussed above provide evidence of a relationship between OOP prices and utilization of opioids. However, these multi-year estimates do not fully exploit the panel nature of the MEPS data. Panel 10 is the only panel of the MEPS that contains observations of the same individuals both before and after the introduction of Part D (years 2005 and 2006). These data allow for the use of individual fixed effects to control for time invariant differences across individuals that may influence their response to Part D. Moreover, this analysis using a single panel of the MEPS allows me to assess whether price-sensitivity differs for new versus existing users. I define new users as those who did not purchase any drug in the relevant category in the year 2005 prior to the implementation of Part D. I define existing users as those who purchased a drug in the relevant category at least one time in the year 2005. I estimate the following fixed effects equation first for Panel 10 pooled, then stratified by new and existing users:

$$Y_{it} = \alpha + \beta(Treatment_i \times Post_t) + \sum \eta_j Age_{it} + \gamma_i + \vartheta_t + \varepsilon_{it}$$

Equation 1-5

where γ_i is an individual-level fixed effects, ϑ_t is an indicator variable for each interview round (each respondent is interviewed a total of 5 times during the two-year period), and all other variables are defined as in Equation 1-2. Standard errors are clustered at the individual level. The coefficient of interest, β, is the estimated impact of Part D within each individual. This estimate is driven by the change in drug utilization for an elderly MEPS respondent

compared to the change for similar non-elderly respondents, before and after the implementation of Part D in 2006.

There are two key differences with this analysis, compared to that presented in Section 1.4. First, new users by definition are those that had a pre-2006 utilization of 0, so I cannot calculate percent changes or elasticities for the new users. Moreover, I cannot observe OOP prices for new users because the MEPS only provides prices for respondents who actually purchased the drug. To ensure that new and existing users both actually experienced OOP price declines after Part D, I estimate a specification of Equation 1-5 in which the outcome variable is the OOP price per *prescription* of *all drugs* (not just painkillers). The finding that new and existing users experienced similar OOP price declines for non-painkillers will increase confidence that they would have experienced similar OOP price declines for painkillers, had I been able to observe them.

Table 1-7 provides estimates from Panel 10. To compare to the baseline results presented in Table 1-4, pre-2006 means and coefficients should be multiplied by 2.5 (i.e. the number of rounds per year). For comparison purposes, the first panel reports estimates of Equation 1-2 using only Panel 10 data. The point estimates (multiplied by 2.5) are similar to earlier estimates in sign and magnitude. However, I find a large gap between new and existing users in their response to Part D. The second panel reports shows that new users experienced a 17 percent reduction in OOP prices and increased their utilization of opioids by 1.79 days supplied per year and non-opioid painkillers by 2.48 painkillers per year. The third panel shows results for existing users. While existing users experienced a statistically significant 21 percent decline in OOP prices, there was no detectable change in their opioid or non-opioid painkiller utilization. These results suggest that it was only the new users who were responsive to Part D's price changes.

1.6 Cross-Price Elasticity Estimates for Over-the-Counter Painkillers with Respect to Prescriptions

Part D presents a useful opportunity to study the degree of substitutability between prescription and OTC painkillers. I first estimate the impact of Part D on utilization of OTC painkillers using the NHCP data and DD models described in Equation 1-2.[22] My outcome variable of interest is the household's total days supplied per year of OTC painkillers, such as Ibuprofen, Naproxen, and Aspirin. I also include household fixed effects on the right hand side to exploit the panel structure of the NHCP. To calculate cross-price elasticities of demand, I divide the estimated percent change of OTC quantity by the percent change of OOP prescription prices from Table 1-4.

Table 1-8 displays the estimated effects of Part D on quantity of OTC painkillers purchased and resulting cross-price elasticities of OTC painkillers with respect to the price of prescription painkillers. I find that Part D led to a 4.3 percent decline in days supplied of OTC painkillers. This implies a positive and statistically significant cross-price elasticity ($\varepsilon = 0.11$), which suggests that consumers view prescriptions and OTC painkillers as substitutes.

I estimate event study models (Appendix Table 1- 16) and sensitivity analyses similar to those described in Section 1.4. In Appendix Table 1- 17, I present results from sensitivity analyses in which I exclude the demographic control variables, estimate models without Nielsen survey weights, omit household fixed effects, and use as my outcome variable an indicator variable for "any OTC purchase." The substantive results are mostly robust to these sensitivity analyses.

[22] I also confirm that Part D did not change prices of OTC drugs for elderly households relative to younger households. Because Part D plans do not cover OTC drugs, the estimated treatment effect of the policy on OTC drug price should theoretically be close to zero and statistically insignificant. Appendix Table 1- 15 confirms that Part D led to no detectable change on OTC prices.

1.7 Discussion

My results help inform the designing of new opioid policies that act through price mechanisms. I find consistent evidence that opioid utilization responds to price stimuli, with a price elasticity of -0.9. Price changes affect the utilization of opioid-naïve individuals, but not of existing opioids users. Moreover, the results for cross-price effects provide evidence of substitution between prescription and OTC painkillers. These findings suggest that opioid-naïve people may be highly responsive to opioid prices and that they likely have substitutes that they are willing to use in place of opioids. Therefore, increasing the OOP price of opioids, through measures such as taxes and formulary design, may be effective in reducing the flow of new opioid use. For example, assuming a policy increases the OOP price of opioids by 10 percent, the per-person opioid consumption would decrease by 9 percent. (This could be understated if demand shifts to the left. Moreover, demand is typically more elastic at higher prices, so as policies increase OOP prices, elasticity may increase.[23]) However, price-based policies will not significantly change utilization among existing users, and so alternative policies are needed to reduce the stock of existing addicts.

In the context of the existing literature, my elasticity result for opioids appears relatively large. Other papers that exploit the introduction of Part D find elasticity estimates for prescription drugs overall ranging from -0.2 to -0.5 (Duggan & Scott Morton, 2010; Ketcham & Simon, 2008; Liu et al., 2011; Yin et al., 2008). Papers that exploit discontinuities in the cost-sharing structure of insurance plans as their empirical design, as opposed to the introduction of Part D, find elasticity estimates that are even more inelastic, ranging from -0.04 to -0.3 (Chandra

[23] Previous studies suggest that the form of the policy matters. Consumers underreact to price changes that are not salient (Chetty, Looney, & Kroft, 2009).

et al., 2010; Coulson & Stuart, 1995; Einav et al., 2018; Hillman et al., 1999; Joyce et al., 2002). However, this discrepancy is likely because my analysis estimates the elasticity of a specific class of drugs, whereas much of the earlier literature estimates the elasticity for prescription drugs overall. Studies show that the impact of prices on drug utilization depends on the therapeutic class of the drug (Goldman et al., 2004), so there is reason to believe that these earlier findings on prescription drugs overall may not apply to pain relief drugs.

Among the few existing studies that identify elasticities separately by drug class, one uses Part D data to study the impact of entering the donut hole on utilization of 150 different types of drugs; for opioids, the authors estimate an elasticity of only -0.04 (Einav et al., 2018). My elasticity estimate for opioids (-0.89) is very different from that of Einav et al. However, this is likely because my paper uses a different identification strategy and answers a different question. Einav et al. exploits within-year price variation around the donut hole, so the sample by definition is limited to people who have spent up to the donut hole (i.e. those who are sicker and therefore have likely used prescription painkillers in the past).[24] Behavioral responses are likely to differ for consumers with different levels of annual drug spending. Moreover, Einav et al.'s aim is to estimate a short-run elasticity of demand with respect to an end-of-the-year increase in the spot price of a drug, and their elasticity estimates are local to the variation used. In the paper, the authors caution that the ordinal ranking of their 150 drug elasticities is more important than the cardinal value of these elasticities.

Another explanation for the high elasticities I estimate is that people view prescription painkillers as substitutes for OTC drugs, at least to some extent. This would mean that people who were previously using more OTC drugs to treat their pain substitute to opioids and other

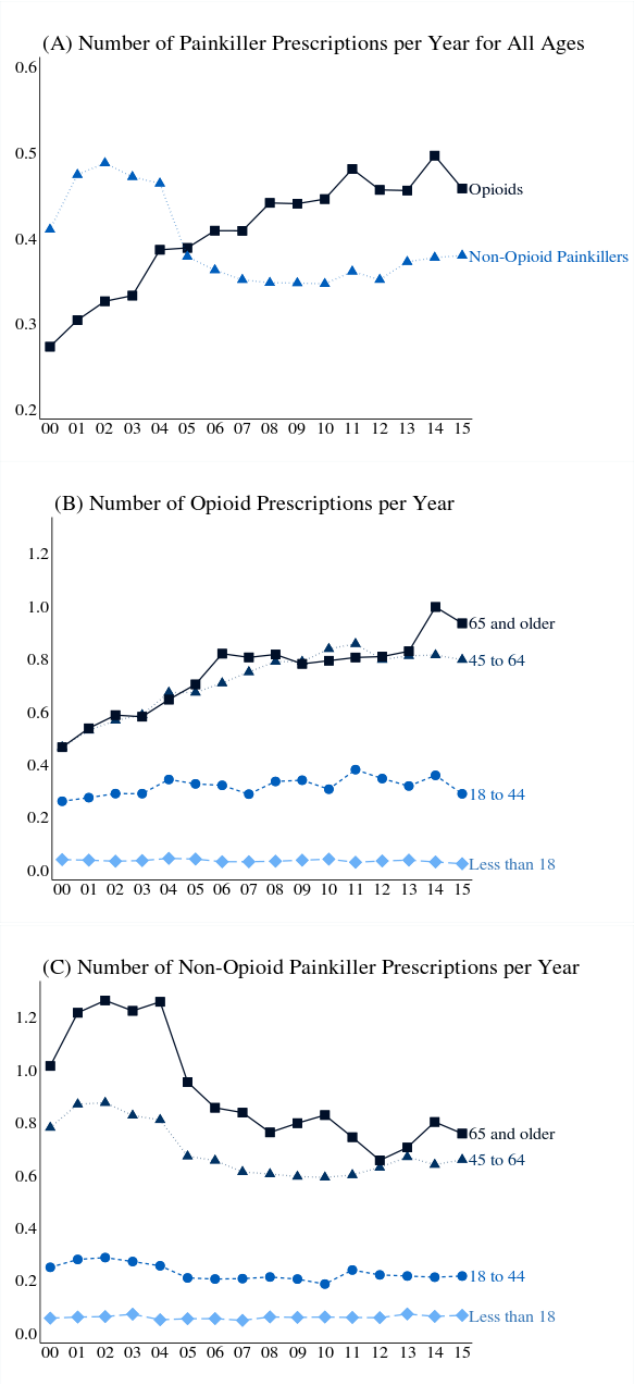
[24] Einav et al. reports that the average Medicare Part D enrollee spends $1,910 on drugs per year, but the spending level to enter the donut hole (i.e. the Einav et al. study sample) is $2,250 to $2,840, which is around the 75th percentile of the expenditure distribution.

prescription painkillers, once their OOP prescription prices drop. Indeed, my analysis of the impact of Part D on OTC purchases shows that cross-price elasticity estimates between prescription and OTC painkillers are significantly positive (elasticity = 0.11).

I acknowledge the limitations of this work. The estimates in this paper are picking up uncompensated responses that are a mix of the price effects, substitution effects, income effects, and information effects of Part D. It is not possible to disentangle these effects with a reduced form model. Nevertheless, my findings provide important evidence that prescription opioids are have a relatively high price elasticity compared to other drugs. This implies that people are sensitive to the price of opioids and that they likely have close substitutes that they are willing to trade off. As such, policies to increase the OOP price of opioids would likely reduce the flow of new opioid use, and the welfare losses associated with such restrictions would likely be small.

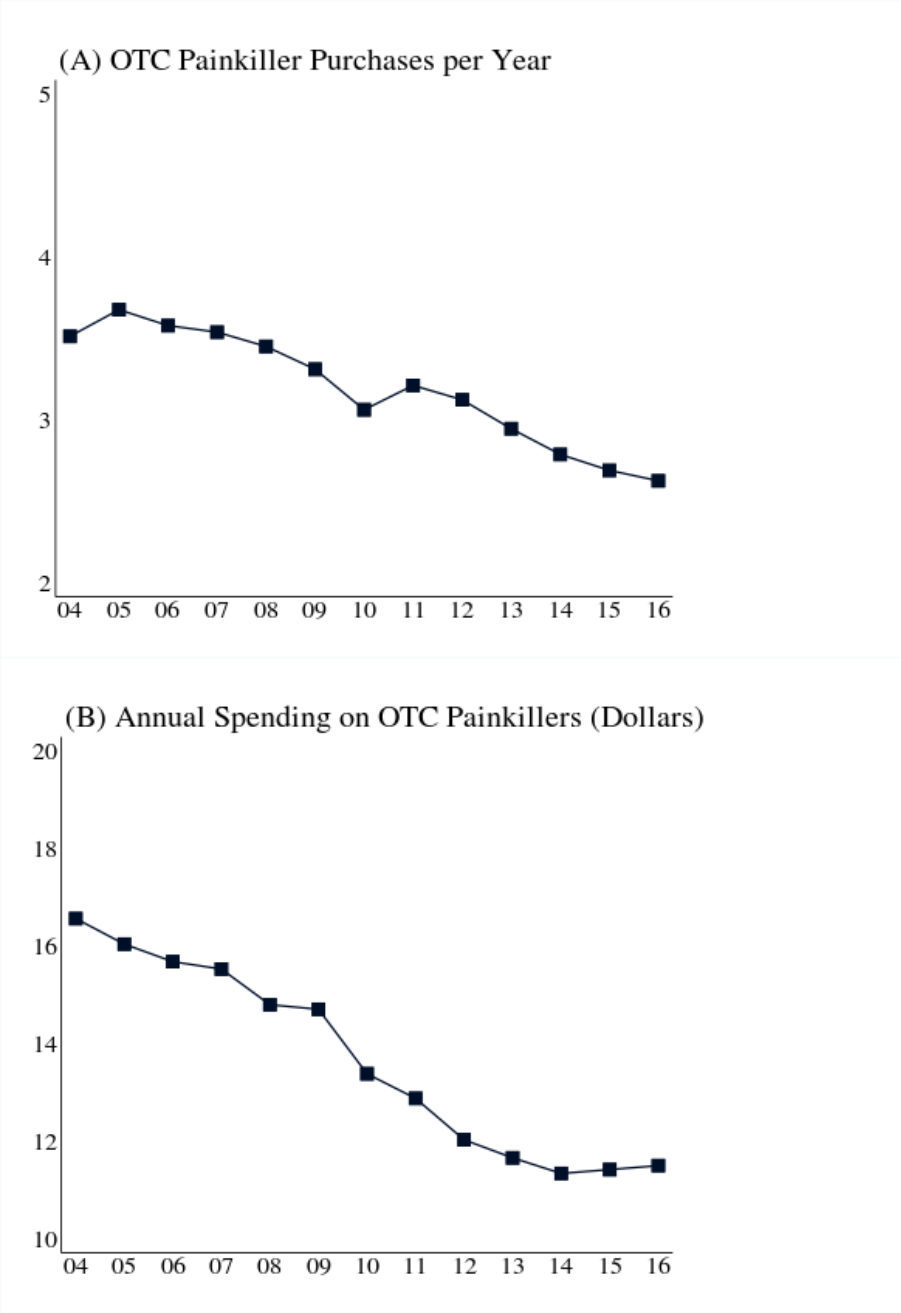
Figures

Figure 1-1. Annual Utilization of Prescription Painkillers per Person



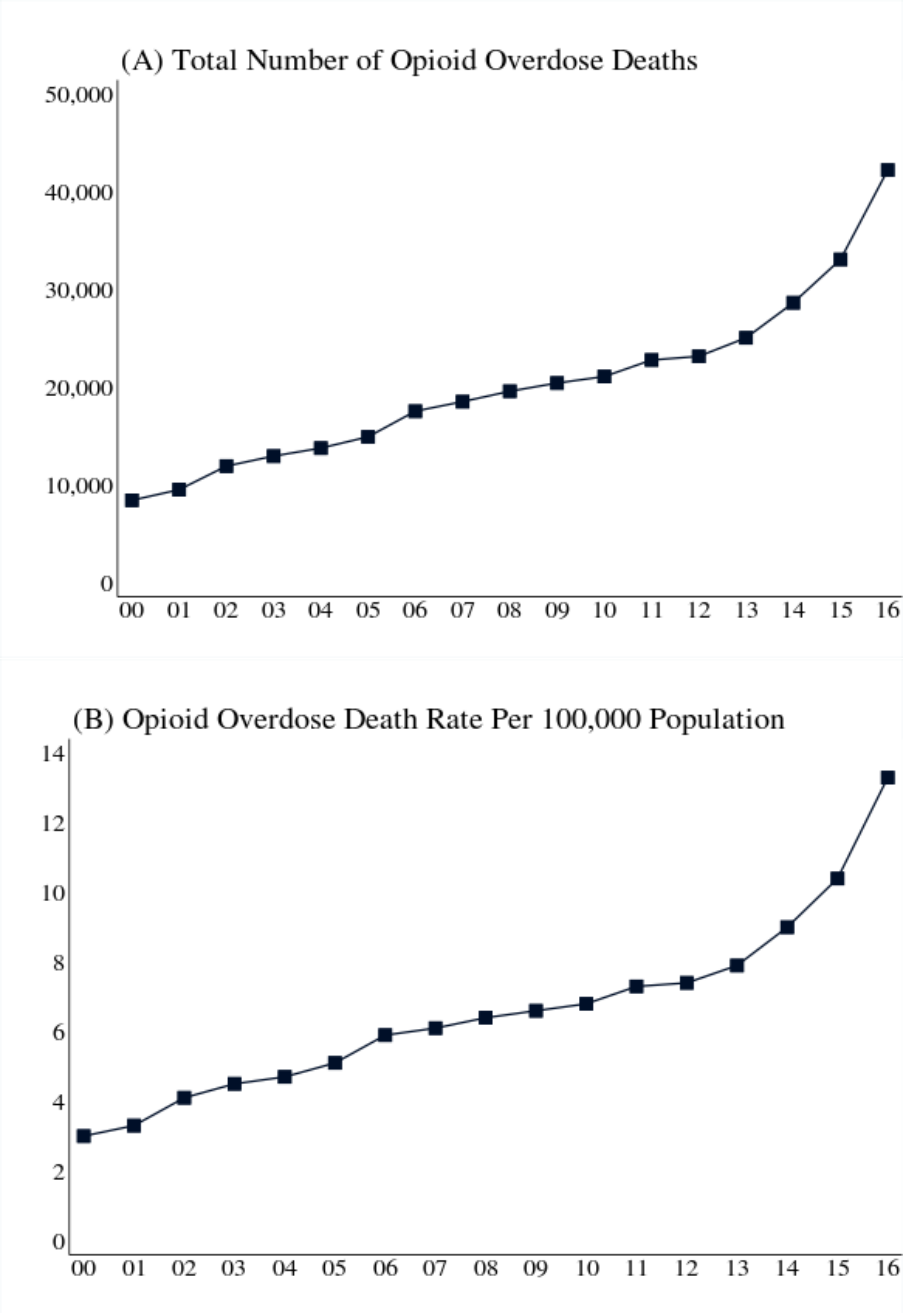
Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2015. Sample is restricted to respondents with non-missing age (N=545,665). Figures display the mean number of painkiller prescriptions per person, adjusted by MEPS survey weights.

Figure 1-2. Annual Utilization of Over-the-Counter Painkillers per Household



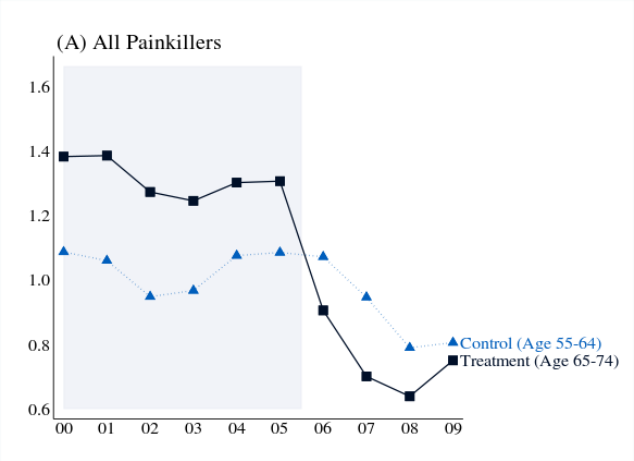
Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2016. Figure displays mean annual spending per household, adjusted by Nielsen survey weights. Spending outcomes has been adjusted for inflation.

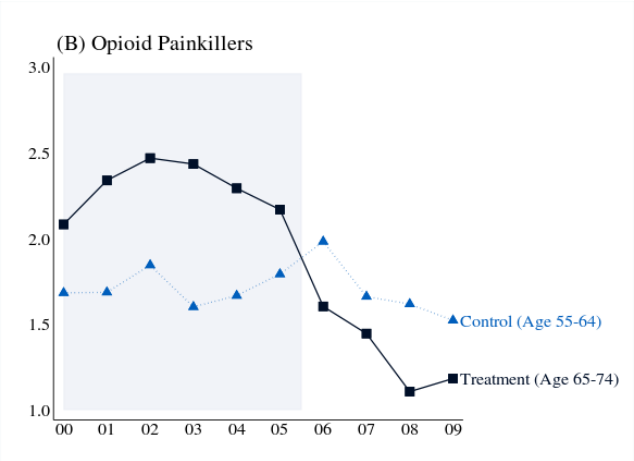
Figure 1-3. Opioid Overdose Deaths

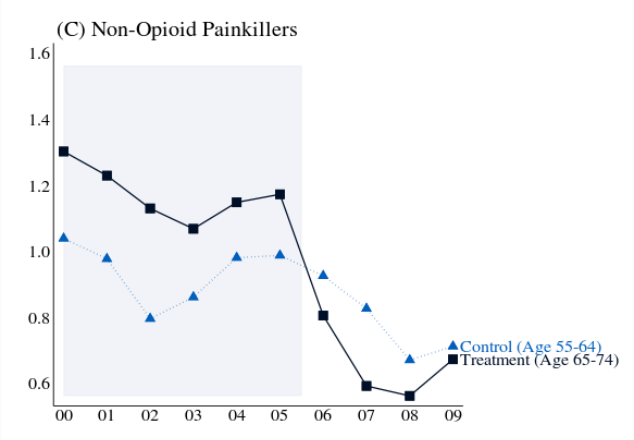


Source: Author's calculations based on data from the Henry J. Kaiser Family Foundation.

Figure 1-4. Out-of-Pocket Prices of Prescription Painkillers over Time

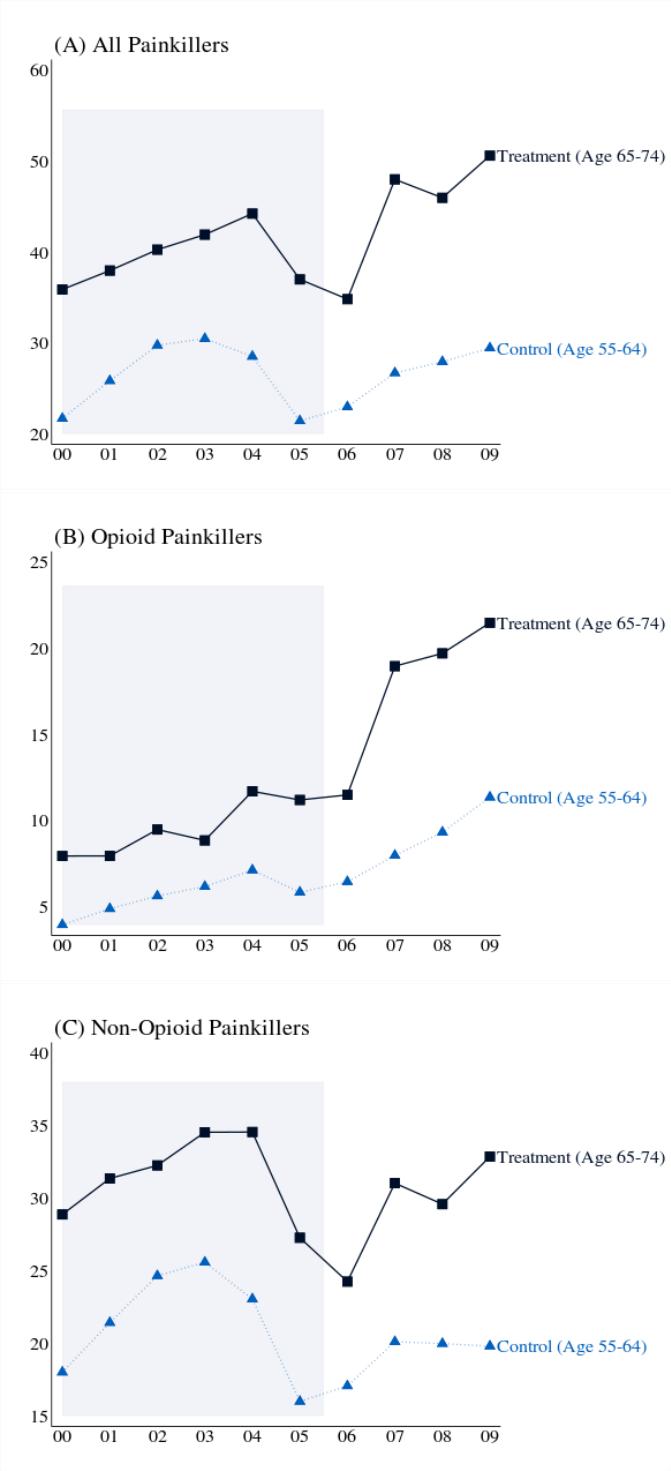






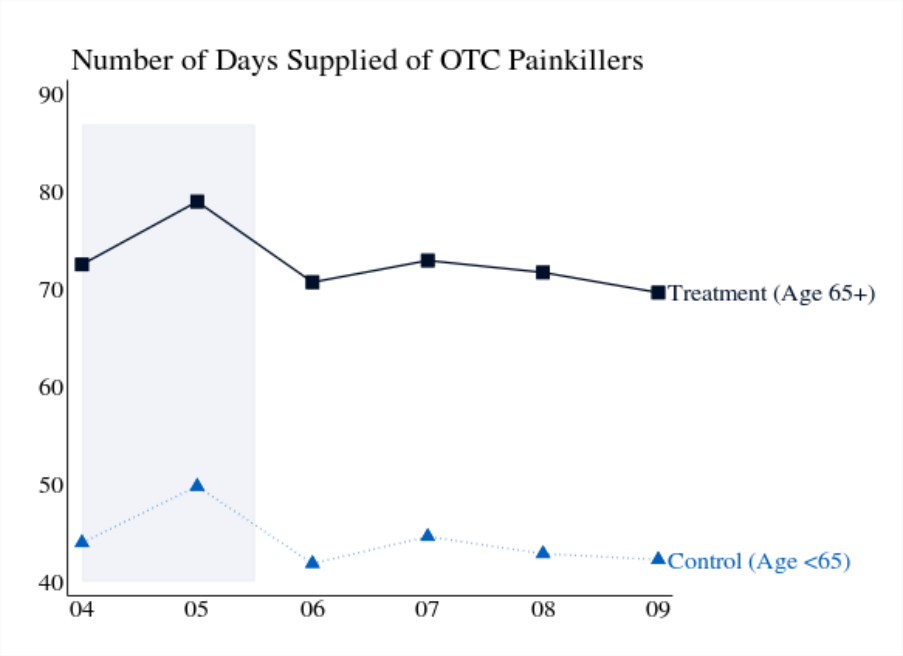
Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Figures display the mean OOP spending per day supply of each NDC, weighted by 2003 utilization of the NDC. Prices are adjusted to 2009 dollars using the Bureau of Labor Statistics' Pharmaceutical Producer Price Index.

Figure 1-5. Utilization of Prescription Painkillers over Time



Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74 (N=50,579). Figures display the mean annual number of days supplied per person, adjusted by MEPS survey weights.

Figure 1-6. Utilization of OTC Painkillers over Time



Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2009. Figure displays the mean annual number of days supplied per household, adjusted by Nielsen survey weights.

Tables

Table 1-1. Descriptive Statistics of the MEPS Sample

	Treatment Group (Ages 65-74) (1)	Control Group (Ages 55-64) (2)	Difference (3)
Male	0.47	0.48	-0.01**
Married	0.63	0.70	-0.07***
Household Income			
Less than 100% FPL	0.10	0.08	0.02***
100 to 124% FPL	0.06	0.02	0.03***
125 to 199% FPL	0.17	0.09	0.08***
200 to 399% FPL	0.30	0.26	0.04***
Greater than 400% FPL	0.38	0.55	-0.17***
Educational Attainment			
Less than high school	0.25	0.14	0.11***
High school	0.35	0.32	0.03***
Some college	0.18	0.22	-0.04***
College or more	0.22	0.32	-0.10***
Race/Ethnicity			
White, Non-Hispanic	0.78	0.77	0.01
Black, Non-Hispanic	0.10	0.09	0.01
Other, Non-Hispanic	0.05	0.05	-0.01**
Hispanic	0.07	0.08	-0.01**
Region			
Northeast	0.19	0.19	-0.00
Midwest	0.22	0.23	-0.01
South	0.39	0.36	0.03***
West	0.20	0.22	0.01**
N	22,265	28,314	

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Means are adjusted by MEPS survey weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1-2. Descriptive Statistics of the Nielsen Sample

	Treatment Group (Ages 65+) (1)	Control Group (Age <65) (2)	Difference (3)
Male Householder	0.61	0.74	-0.13***
Female Householder	0.80	0.81	-0.01***
Married	0.38	0.52	-0.14***
Household Income			
Less than 100% FPL	0.05	0.07	-0.01***
100 to 124% FPL	0.04	0.03	0.01***
125 to 199% FPL	0.17	0.11	0.05***
200 to 399% FPL	0.41	0.30	0.10***
Greater than 400% FPL	0.33	0.48	-0.15***
Educational Attainment of Male Householder			
No male householder	0.39	0.26	0.13***
Less than high school	0.08	0.05	0.03***
High school	0.24	0.24	0.00
Some college	0.15	0.22	-0.07***
College or more	0.14	0.23	-0.10***
Educational Attainment of Female Householder			
No female householder	0.20	0.19	-0.01***
Less than high school	0.07	0.03	0.04***
High school	0.39	0.26	0.12***
Some college	0.22	0.27	-0.04***
College or more	0.12	0.25	-0.13***
Race/Ethnicity			
White, Non-Hispanic	0.82	0.72	0.10***
Black, Non-Hispanic	0.09	0.12	-0.03***
Other, Non-Hispanic	0.03	0.05	-0.02***
Hispanic	0.06	0.10	-0.05***
Region			
Northeast	0.21	0.20	0.01**
Midwest	0.24	0.25	-0.00
South	0.33	0.32	0.01**
West	0.22	0.23	-0.01***
N	97,276	237,784	

Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2009. Means are adjusted by Nielsen survey weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1-3. Classification of Prescription Painkillers in MEPS

	Examples (1)	Any Prescription (2)	Prescriptions Per Year (3)	Total Price Per Prescription (4)	OOP Price Per Prescription (5)
All Drugs		0.90	27.91	68.07	27.77
All Painkillers		0.35	1.96	47.08	18.86
Opioids					
All Opioids		**0.19**	**0.87**	**32.36**	**13.23**
Hydrocodone	Vicodin, Lortab, Lorcet	0.08	0.30	19.43	9.32
Propoxyphene	Darvocet, Darvon, Propacet	0.04	0.15	25.61	13.74
Oxycodone	Oxycontin, Percocet, Endocet	0.04	0.14	51.81	17.04
Tramadol	Ryzolt, Ultram, Ultracet	0.02	0.12	43.29	18.12
Codeine	Codeine & Tylenol	0.02	0.06	15.63	8.35
Morphine	MS Contin, Kadian, Avinza	0.01	0.03	71.08	19.36
Fentanyl	Duragesic, Actiq	0.00	0.03	249.83	75.58
Methadone	Methadose, Dolophine	0.00	0.02	27.17	11.33
Other Opioids	Hydromorphone, Meperidine, Pentazocine, Dihydrocodeine	0.00	0.01	35.05	18.74
Non-Opioid Painkillers					
All Non-Opioid Painkillers		**0.23**	**1.09**	**57.56**	**23.05**
Acetylsalicylic Acid	Aspirin, Ecotrin	0.04	0.20	7.71	3.63
Celecoxib	Celebrex	0.05	0.20	122.18	45.38
Rofecoxib	Vioxx	0.03	0.09	82.17	40.69
Diclofenac	Arthrotec, Voltaren	0.02	0.09	64.05	25.25
Ibuprofen	Advil, Motrin	0.03	0.09	17.54	7.49
Naproxen	Aleven, Naprelan, Anaprox	0.03	0.09	42.38	15.80
Meloxicam	Mobic	0.02	0.07	78.31	32.15
Acetaminophen	Tylenol, Fioricet, Mapap	0.02	0.06	12.74	6.59
Nabumetone	Relafen	0.01	0.04	60.97	26.80
Valdecoxib	Bextra	0.01	0.03	103.45	61.56
Indomethacin	Indocin	0.01	0.03	31.39	11.66
Etodolac	Lodine	0.01	0.03	47.16	15.66
Piroxicam	Feldene	0.00	0.02	46.80	17.14
Sulindac	Clinoril, Disalcid	0.00	0.01	46.31	18.05
Oxaprozin	Daypro	0.00	0.01	54.21	17.00
Other Non-Opioid Painkillers	Sumatriptan, Salsalate, Ketoprofen	0.01	0.03	92.99	25.67
N		22,265	22,265	22,265	22,265

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. The data in the last four columns displays means for the treatment group (elderly individuals) across all years, adjusted by MEPS survey weights.

Table 1-4. DD Results and Own-Price Elasticity Estimates for Prescription Painkillers

	Utilization (Days Supplied)			Price (OOP Price per Day Supplied)			Elasticity
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)	Pre-2006 Mean (4)	DD Coefficient (5)	Percent Change (6)	(7)
Painkillers							
All Painkillers	39.57	4.33* (2.29)	10.9%	1.34	-0.51*** (0.13)	-38.1%	-0.29
Opioids	9.54	4.81*** (1.23)	50.4%	2.31	-1.30** (0.56)	-56.5%	-0.89
Non-Opioid Painkillers	31.50	0.02 (2.03)	-	1.17	-0.40*** (0.14)	-34.2%	-
Opioids, by Dosage							
Total MME	540.10	401.82** (162.18)	74.4%	0.03	-0.02** (0.01)	-66.7%	-1.12
High Dose Opioids	1.58	0.77* (0.47)	48.7%	5.41	-4.31 (3.67)	-	-
Low Dose Opioids	8.04	4.72*** (1.16)	58.7%	1.73	-0.72** (0.31)	-41.6%	-1.41
Opioids, by Release							
Extended Release Opioids	2.04	1.54** (0.72)	75.5%	2.98	-0.57 (1.55)	-	-
Immediate Release Opioids	7.71	4.03*** (1.00)	52.2%	1.84	-1.02** (0.51)	-55.4%	-0.95
N		50,579			3,454		

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Columns 1-3 are based on results from Equation 1-2. Sample is restricted to adults aged 55 to 74. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, column 3 displays percent change from pre-2006 mean.

Columns 4-6 based on results from Equation 1-3. Sample is restricted to painkiller NDCs for which at least one year of pre-2006 and one year of post-2006 data is available. Column 4 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions include a treatment group indicator and year fixed effects. Data are weighted by 2003 level of utilization of the NDC. For statistically significant point estimates, column 6 displays percent change from pre-2006 mean.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1-5. Event Study Results for Prescription Painkillers

	Utilization (Days Supplied)			Price (OOP Price per Day Supplied)		
	All Painkillers (1)	Opioids (2)	Non-Opioid Painkillers (3)	All Painkillers (4)	Opioids (5)	Non-Opioid Painkillers (6)
Year 2000 X Treatment	-1.47 (3.62)	-1.26 (1.83)	-0.55 (3.38)	0.30 (0.27)	0.40 (0.45)	0.26 (0.30)
Year 2001 X Treatment	-3.98 (3.42)	-2.15 (1.53)	-1.96 (3.02)	0.33 (0.33)	0.65 (0.90)	0.25 (0.31)
Year 2002 X Treatment	-5.61* (3.29)	-1.45 (1.77)	-4.24 (2.95)	0.32 (0.22)	0.62 (0.88)	0.33 (0.24)
Year 2003 X Treatment	-4.65 (3.71)	-2.82 (1.79)	-2.69 (3.26)	0.28 (0.30)	0.83 (0.88)	0.21 (0.37)
Year 2004 X Treatment	0.09 (3.11)	-0.62 (1.57)	0.05 (2.97)	0.23 (0.21)	0.63 (0.94)	0.17 (0.26)
Year 2006 X Treatment	-4.79 (3.15)	-0.70 (1.68)	-4.85* (2.69)	-0.17 (0.25)	-0.38 (1.21)	-0.12 (0.25)
Year 2007 X Treatment	5.03 (4.06)	5.20** (2.11)	-0.67 (3.46)	-0.25 (0.26)	-0.22 (1.03)	-0.23 (0.27)
Year 2008 X Treatment	1.55 (4.35)	4.50* (2.39)	-2.10 (3.69)	-0.15 (0.16)	-0.51 (0.48)	-0.11 (0.22)
Year 2009 X Treatment	4.85 (4.11)	4.50** (2.19)	1.27 (3.50)	-0.06 (0.23)	-0.34 (0.56)	-0.04 (0.30)
p-value for test that all pre-2006 terms jointly equal 0	0.57	0.48	0.79	0.26	0.68	0.54
N	50,579	50,579	50,579	3,454	1,664	1,790

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Table displays the coefficient on the interaction of the treatment group indicator and each year indicator. The year 2005 is omitted as the base year. Regressions in columns 1-3 control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. Regressions in columns 4-6 include a treatment group indicator and year fixed effects. Data are weighted by 2003 level of utilization of the NDC.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1-6. Heterogeneous Effects for Impact of Part D on Prescription Painkiller Utilization by Reported Condition

	Individuals with the condition			Individuals without the condition		
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)	Pre-2006 Mean (4)	DD Coefficient (5)	Percent Change (6)
Cancer						
All Painkillers	43.31	4.39 (5.96)	-	38.97	3.84 (2.45)	-
Opioids	14.13	6.36** (3.23)	45.0%	8.80	4.31*** (1.33)	49.0%
Non-Opioid Painkillers	30.91	-2.29 (5.19)	-	31.59	0.13 (2.15)	-
N		5,069			45,510	
Joint or Back Pain						
All Painkillers	76.66	8.52* (4.61)	11.1%	15.62	-1.43 (1.57)	-
Opioids	19.05	7.05*** (2.72)	37.0%	3.40	0.44 (0.71)	-
Non-Opioid Painkillers	61.04	2.13 (4.02)	-	12.43	-1.94 (1.46)	-
N		18,813			31,766	
Poisoning by medical and non-medical substances						
All Painkillers	62.24	-3.63 (21.89)	-	39.15	4.51** (2.27)	11.5%
Opioids	21.75	6.33 (14.04)	-	9.32	4.87*** (1.22)	52.3%
Non-Opioid Painkillers	45.42	-18.55 (19.99)	-	31.24	0.27 (2.03)	-
N		663			49,916	

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Columns 1 and 4 display the pre-2006 mean for the treatment group. Columns 2 and 5 display the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, columns 3 and 6 displays percent change from pre-2006 mean.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1-7. Heterogeneous Effects for Impact of Part D on Prescription Painkiller Utilization for New vs. Existing Users

	Utilization (Days Supplied)			Price (OOP Price per Prescription)		
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)	Pre-2006 Mean (4)	DD Coefficient (5)	Percent Change (6)
Pooled Sample						
All Painkillers	13.83	2.70** (1.22)	19.5%			
Opioids	4.11	1.46** (0.68)	35.5%			
Non-Opioid Painkillers	9.92	1.17 (0.99)	-			
All Drugs				31.92	-5.99*** (1.24)	-18.8%
N		12,068			12,068	
New Users						
All Painkillers	0.00	3.62*** (0.62)	-			
Opioids	0.00	1.79*** (0.39)	-			
Non-Opioid Painkillers	0.00	2.48*** (0.44)	-			
All Drugs				33.59	-5.86*** (1.38)	-17.4%
N		8,791			8,791	
Existing Users						
All Painkillers	48.66	1.58 (3.93)	-			
Opioids	24.89	1.52 (3.88)	-			
Non-Opioid Painkillers	57.49	-3.16 (5.46)	-			
All Drugs				30.76	-6.57*** (2.43)	-21.4%
N		3,277			3,277	

Source: Author's calculations based on Medical Expenditure Panel Survey 2005 to 2006. Sample is restricted to adults aged 55 to 74. Columns 1 and 4 display the pre-2006 mean for the treatment group. Columns 2 and 5 display the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, columns 3 and 6 displays percent change from pre-2006 mean.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1-8. DD Results and Cross-Price Elasticity Estimates for OTC Painkillers

	Utilization (Days Supplied)			Cross-Price Elasticity
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)	
OTC Painkillers	75.68	-3.27*** (1.01)	-4.3%	0.11
N		335,060		

Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2009. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for householder's sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include household fixed effects and year fixed effects. Data are adjusted by Nielsen survey weights, and standard errors are clustered on household.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2 Increasing Pharmaceutical Access for the Elderly Improves Functional Outcomes: Implications for Caregivers

Abstract

One in four elderly Americans faces some limitation in activities of daily living (ADL), such as bathing, eating, or walking across the room. People with such functional limitations also have substantially larger long-term care needs; they often rely on informal caregiving from spouses and family members, which may reduce caregivers' labor force participation. This study assesses the impact of increased pharmaceutical access on elderly individuals' functional outcomes by exploiting increased prescription drug availability brought on by the introduction of Medicare Part D in 2006. I use nationally representative individual-level survey data and find that Part D improved functional outcomes among elderly beneficiaries and reduced their reliance on informal caregiving. Further analysis with time use data shows that Part D reduced the amount of time that non-elderly spouses and children spend providing care to older individuals, freeing up their time to spend on other activities. My results suggest that increasing pharmaceutical access for the elderly not only improves their own functional outcomes, but also has positive spillover effects for non-elderly caregivers.

2.1 Introduction

Functional outcomes – defined as the capacity to perform activities of daily living (ADLs) without significant difficulties – are important determinants of individuals' ability to live independent, healthy, and productive lives. Functional outcomes typically include five ADLs – bathing, dressing, eating, walking across a room, and getting in and out of bed – and five instrumental activities of daily living (IADLs) – making phone calls, managing money, grocery shopping, taking medications, and preparing hot meals. The ability to perform these activities without difficulty diminishes as people age, due to arthritis, osteoporosis, joint/nerve pain, heart disease, diabetes, cognitive decline, chronic respiratory illness, renal/urinary tract illness, and other medical conditions associated with aging (Malhotra, Chan, Malhotra, & Ostbye, 2012). In the United States, nearly one in every four individuals over age 65 has at least some difficulty performing one or more of the ten ADLs/IADLs. Although functional limitations are most common among those above age 80, they are present even among younger seniors, with 16 percent of those aged 65 to 69 facing at least one functional limitation (Table 2-1). Those who have less educational attainment, have lower income, are not married, are women, are Black or Hispanic, or are veterans are more likely to face poor functional outcomes. Conditional on having any limitation, the average elderly person faces three ADL/IADL limitations; dressing, bathing, grocery shopping, and managing money are the activities that most elderly adults have difficulty with.

The high prevalence of functional limitations is concerning because poor functional outcomes can reduce individuals' labor force participation, worsen their health outcomes, and increase their reliance on long-term care. Those with ADL limitations are more likely to need in-home and nursing home care, and Medicaid is the largest payer for this care (Brown &

Finkelstein, 2011). Moreover, seniors with ADL limitations often rely on informal (unpaid) caregiving from spouses and relatives (Feldman et al., 2003), which has high social costs including reduced labor force participation and higher levels of stress and anxiety among caregivers (American Psychological Association, 2006; Lilly, Laporte, & Coyte, 2007). The aging American population will only intensify these problems in the future, as the proportion of elderly people requiring long-term care increases to working-age people increases. The old-age dependency ratio is expected to grow dramatically from 29 elderly people per 100 working-age people in 2020 to 40 per 100 by 2060 (US Census Bureau, 2018).

There are many prescription drugs available for osteoarthritis, back pain, and osteoporosis, all potential causes of decreased ADL. However, these drugs can be quite expensive for those who lack prescription drug insurance. For example, the average list prices for the most common drugs to treat arthritis are $11 per day supplied for Rheumatrex, $5 for Plaquenil, and $3 for generic Methotrexate; the list prices for the most common back pain drugs are $4 per day supplied for Celecoxib, $3 for Diclofenac, and $1 for Aspirin (www.drugs.com). It is plausible that policy efforts to reduce patients' out-of-pocket (OOP) drug prices may increase drug utilization and therefore improve functional outcomes.

In this study, I assess the extent to which prescription drug coverage affects seniors' functional outcomes and dependence on informal caregiving. My empirical identification comes from the introduction of Medicare Part D in January 2006, a policy that provided publicly subsidized prescription drug coverage to Medicare eligibles (mostly those above the age of 65). Part D reduced the fraction of drug uninsured elderly from 24 percent to 7 percent in its first year and substantially reduced OOP drug prices for the elderly (Ketcham & Simon, 2008; Levy & Weir, 2009). The policy has been used extensively to study causal effects of prescription drug

coverage on health and labor market outcomes; Appendix Table 2- 1 provides a comprehensive review of the Part D literature.

While a number of studies find that Part D improved pharmaceutical access and increased drug utilization, there has been surprisingly little research of the impacts of Part D on health outcomes. Studies on mortality find conflicting results: some find reductions in cardiovascular deaths (Dunn & Shapiro, 2019; Huh & Reif, 2017) and another finds no significant impact of the policy on mortality (Kaestner, Schiman, & Alexander, 2017). The few studies that do evaluate non-mortality health outcomes focus on clinical outcomes rather than functional outcomes. Research shows that Part D improved mental health (Ayyagari & Shane, 2015), reduced chronic pain (Ayyagari, 2016), increased measures of self-assessed health (Chen, Lin, & Seo, 2018) and reduced inpatient hospitalizations (Afendulis, He, Zaslavsky, & Chernew, 2011; Kaestner et al., 2017; Zhang, Donohue, Lave, O'Donnell, & Newhouse, 2009). One study using data predating Part D finds that prescription drug insurance has no detectable effect on functional disability for the overall elderly population, but does improve functional outcomes for older seniors above age 71, those with chronic illness, and those who obtained drug coverage through a Medicare HMO (Khan, Kaestner, & Lin, 2008). While Khan et al. provides suggestive evidence that Part D may have reduced ADL limitations, the results may be biased if before 2006, elderly individuals sought prescription drug coverage based on unobservable factors correlated with functional outcomes.

There is only one study I am aware of that directly studies the impact of Part D on functional outcomes (Chen et al., 2018). Chen et al. use the Health and Retirement Study's (HRS) Prescription Drug Study and find improvements in self-assessed health but no detectable effect on ADL limitations. My analysis builds on this study in several ways. First, Chen et al. use

a small sample of about 750 individuals so low statistical power may impair their ability to find significant results. In contrast, I use the entire HRS sample and have nearly 92,000 observations in my analysis. Second, Chen et al.'s empirical strategy relies on prescription drug enrollment (not eligibility); respondents with a new and continuous enrollment of the Part D from 2006 to 2008 comprise the treatment group, and people who were never enrolled in Part D during 2006–2008 comprise the control group. This approach may bias coefficient estimates downward if those who chose to enroll in Part D were individuals who were in worse health. My empirical approach, on the other hand, exploits changes in *eligibility* generated by Part D. Finally, Chen et al. use only one wave of pre-2006 data and two waves of post-policy data, whereas I use data spanning 1996 to 2010, which allows me to assess potential dynamic long-term effects as well as parallel trends between the treatment and control group in the pre-policy period.

By providing some of the first evidence of Part D's effects on functional outcomes, this paper fills an important gap in the literature. Though functional outcomes may not seem as immediately life-threatening as some clinical outcomes, they are key determinants of elderly individuals' daily functioning, long-term health, and reliance on formal and informal long-term care. I provide suggestive evidence on how pharmaceutical access for the elderly affects their utilization of informal unpaid care by evaluating the impact of Part D on time spent caregiving among non-elderly household members. Though there are various sources of formal and informal long-term care,[25] in this paper, I focus on informal (unpaid) caregiving because it is the most common type of care and comes with high social costs. Surveys show that 64 percent of

[25] There are four main sources of long-term care. Informal care is the unpaid care provided by family and friends, whereas formal care is paid and includes in-home health workers, assisted living facilities, and nursing homes. Informal care, in-home health workers, and assisted living facilities provide mostly custodial and basic medical care. These forms of long-term care are most often associated with adverse functional outcomes. Nursing home care is meant for individuals with complex health conditions, such as Alzheimer's and dementia, who require round-the-clock monitoring or medical care.

elderly individuals rely exclusively on informal care, 28 percent rely on both formal and informal care, and only 8 percent rely solely on formal care (Georgetown Health Policy Institute, 2005). Informal care is most often provided by female spouses and adult daughters. Over 14 percent of nonelderly women and 11 percent of nonelderly men spend at least some time caring for other adults; on average, caregivers spend about 5 hours per week providing physical and medical care to adults (Appendix Figure 2- 2).

Although informal care is "unpaid" by definition, it has several social costs that are of concern to policymakers. The largest is the opportunity cost of the time that could otherwise be spent on labor and leisure activities. Studies find that informal caregivers work 1.2 hours per week less and enjoy 4.0 hours less leisure time than non-caregivers (Kydland & Pretnar, 2019). The economic value of services provided by informal caregivers totals about $470 billion per year (Reinhard, Feinberg, Choula, & Houser, 2015). Moreover, caregivers report higher levels of stress, anxiety, and depression (American Psychological Association, 2006), and 10 percent of caregivers have competing childcare responsibilities (Georgetown Health Policy Institute, 2005).

This paper provides some of the first evidence of the impact of expanded pharmaceutical access on informal caregiving. I find that Part D reduced the probability that a nonelderly adult spent time caregiving by nearly 20 percent. Much of this reduction can be attributed to improved functional outcomes among the elderly. I find that Part D reduced the number of ADL/IADL limitations by 13 percent and the probability of having any functional limitation by 10 percent. This research has important economic and public health implications. Seniors with fewer ADL limitations are more likely to engage in community activities such as labor and community service; to the extent that it increases their retirement income, this increased engagement may reduce the burden on government welfare programs. Healthier seniors also have the ability to

live independently, freeing up spouses and other informal caregivers to participate in the labor force or pursue leisure activities.

My results are particularly relevant in light of recent policy changes that may affect prescription drug availability. First, a provision of the Affordable Care Act (ACA) enacted the Part D coverage gap phase out and closure by the year 2020. Elderly individuals will likely increase their prescription drug utilization in coming years as their out-of-pocket costs reduce, and it is important for policymakers to understand the health and economics implications of this increased utilization. Second, reducing drug prices is a top priority for the federal government; there is ongoing discussion about promoting use of generics, reference pricing, and potentially allowing CMS to negotiate directly with drug manufacturers. Even after Part D, many seniors, particularly those with multiple chronic conditions, report difficulty paying for their medications (Naci et al., 2014). To the extent that proposed price cuts increase drug utilization among seniors, they will not only improve functional outcomes among the elderly but may also help reduce the burden of informal care among the nonelderly.

2.2 Conceptual Framework

My analysis focuses on two groups of agents: elderly individuals and non-elderly individuals (potential caregivers). I assume that elderly people are trying to maximize their functional outcomes, which can be improved through increased consumption of prescription drugs and non-drug medical care. Several medical studies show that certain prescription drugs can reduce ADL/IADL limitations (Cañete et al., 2006; Feldman et al., 2003; Hamilton, Brydson, Fraser, & Grant, 2001). Seniors decide their optimal quantity of drug consumption based on their OOP prices of drugs and non-drug substitutes, income, information received from physicians, and other factors. Part D affected this choice by increasing the portion of income that

could be allocated to non-medical spending (Ayyagari & He, 2016; Engelhardt & Gruber, 2011) and lowering OOP prices of drugs used to treat arthritis, heart disease, diabetes, joint/nerve pain, and other potential causes of ADL limitations (Duggan & Scott Morton, 2010; Liu et al., 2011). Both these phenomena increased drug consumption among the elderly (Ketcham & Simon, 2008); in particular, antihypertensives, lipid regulators, diabetes drugs, and analgesics were among the top ten therapy classes of prescription drugs prescribed in Part D (Engelhardt & Gruber, 2011). Increased drug utilization should in turn improve functional outcomes.

The second set of agents consists of non-elderly people who are potential caregivers. They divide their time between labor, leisure, and caregiving. The amount of time they spend on caregiving depends on the health of their elderly family members; better functional and clinical outcomes for the elderly implies less time spent on informal caregiving among the non-elderly. Non-elderly agents are subject to a time constraint, which means that any time spent caregiving takes away time that could be spent on labor and leisure. Part D affects the caregiver's decision by improving elderly people's clinical (Afendulis et al., 2011; Ayyagari & Shane, 2015; Chen et al., 2018; Kaestner et al., 2017; Zhang et al., 2009) and functional outcomes. This should theoretically reduce time spent caregiving.

However, it is also important to consider the mortality effects of Part D. Studies have found that Part D reduced cardiovascular mortality (Dunn & Shapiro, 2019; Huh & Reif, 2017). This implies that pharmaceutical access increases the number of elderly people who are alive, which might increase total caregiving needs. Ultimately, the effect of prescription drug coverage on informal caregiving is an empirical question.

2.3 Data and Measures

This study uses two main data sources: the Health and Retirement Study (HRS) and the American Time Use Survey (ATUS).

The HRS is a nationally representative panel of adults over age 50 and their spouses (Health and Retirement Study, 2014). It is conducted biennially from 1992 to 2016 and contains information on respondents' prescription drug coverage and health outcomes. The sample size is about 18,000 to 20,000 individuals each year. The survey contains detailed information on individuals' health outcomes, ADL and IADL limitations, and labor force status. For this analysis, I use data from the 1996 to 2012 waves of the Rand HRS file ("RAND HRS Fat File Version P," 2014);[26] earlier waves are missing data on certain functional limitations, and in later years, the Affordable Care Act (ACA), which expanded insurance coverage for the non-elderly in 2014, may confound results. My main analyses are further restricted to individuals aged 55-74 with non-missing data for my outcomes and covariates of interest. The sample size is 103,755 across all waves. Appendix Table 2- 2 contains demographic characteristics of the HRS study sample.

I examine six key outcomes from the HRS: (1) Number of ADLs (bathing, dressing, eating, getting in and out of bed, and walking across the room) that the respondent has at least some difficulty with; (2) Number of IADLs (talking on the telephone, managing money, taking medications, shopping for groceries, and preparing meals) that the respondent has at least some difficulty with; (3) total number of ADLs and IADLs that the respondent has at least some difficulty with; (4) whether the respondent has any ADL limitations; (5) whether the respondent

[26] The HRS (Health and Retirement Study) is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan. The RAND HRS Fat Files take almost all the raw variables from the HRS survey, and collapse them into a single respondent-level dataset for each wave. This file was developed at RAND with funding from the National Institute on Aging.

has any IADL limitations; (6) and whether the respondent has any ADL or IADL limitations. In supplementary analyses, I also examine the effect of Part D on other health and labor outcomes, such as gross motor limitations, self-assessed health, prescription drug utilization, doctor visits, and home health care utilization.

My data source for the caregiving analysis is the ATUS, a survey that collects information on time spent on various activities each day. The ATUS is conducted by the US Census Bureau every year from 2003 to 2017 and consists of a nationally representative random sample of about 10,000 individuals per year. The sample is drawn from a set of households that complete their final interview for the Current Population Survey (CPS). Respondents sequentially report activities they perform between 4 am on the day before the interview until 4 am on the day of the interview. Respondents are interviewed on both weekend days and weekdays. Sampling weights are calculated to provide representative full-week estimates of the non-institutionalized US population. I also merge in information on presence and age of other people in the respondent's household from the CPS. For my main analysis, I restrict the sample to adults aged 27 to 64 who completed their ATUS interviews between 2003 and 2013. I limit my sample to those 27 and older because young adults below age 26 became eligible for the dependent coverage provision of the ACA during my sample period, which may have influenced their household formation decisions (Abramowitz, 2016). The sample size is 101,423 across all years. Appendix Table 2- 3 provides demographic characteristics of the ATUS study sample.

The Census Bureau groups reported activities into 17 major categories and dozens of subcategories; my key outcome variable of interest is time spent "caring for and helping household adults." This includes physical care for household adults, looking after household adults (as a primary activity), providing medical care to household adults, obtaining medical and

care services for household adults, helping household adults, organization and planning for household adults, and picking up or dropping off household adults. Although the ATUS does not specify whether the care was provided for an elderly adult or nonelderly adult, one study finds that the ATUS measure of "caring for and helping household adults" is nearly identical to "caring for and helping infirm elders in the household (Kydland & Pretnar, 2019). In supplementary analysis, I also study the effect of Part D on non-elderly adults' labor force status and other daily activities, including housework, volunteer, and working.

2.4 Methods

My empirical objective is to estimate the effects of prescription drug coverage on functional outcomes and caregiving. The main obstacle to obtaining unbiased causal estimates is endogeneity; simply calculating the correlation between drug utilization and health outcomes does not inform the direction of causality. Moreover, there may be confounding factors that affect both drug utilization as well as health outcomes. A reasonable alternative method may be to use prescription drug coverage as an instrument for drug utilization, as there is substantial empirical evidence to show that obtaining drug insurance increases the use of prescription drugs. However, simply comparing drug uninsured with drug insured individuals would not yield causal estimates either because of selection: people who are in worse health are more likely to enroll in generous insurance plans as well as have worse health outcomes; this would bias the estimate downwards.

In order to estimate the causal effects of drug coverage, I exploit a natural experiment that created an exogenous change in prescription drug insurance for a treatment group of elderly people – the introduction of Medicare Part D in 2006. In contrast to insurance plans that individuals select and fully pay for themselves, Part D plans were available at highly subsidized

rates to all Medicare-eligible adults and are therefore less likely to be correlated with other factors that affect the demand for drugs. There is a large literature establishing the first stage of my analysis, that Part D increased utilization of prescription drugs (Ketcham & Simon, 2008; Lichtenberg & Sun, 2007; Yin et al., 2008). I estimate the downstream effects of this increased drug utilization on functional outcomes and caregiving using a difference-in-differences (DD) approach.

2.4.1 Functional Outcomes

I first use HRS data to estimate DD models that compare outcomes among people aged 65-74 (the treatment group) with individuals aged 55-64 (the control group), before and after the implementation of Medicare Part D in 2006. The use of the near-elderly control group helps separate Part D's effects from other secular factors that may have changed at the same time (e.g. drugs going off patent). My baseline model is:

$$Y_{igt} = \alpha + \beta\left(Treatment_g * Post_t\right) + \gamma \boldsymbol{X}_{igt} + \eta_g + \vartheta_t + \varepsilon_{igt} \quad (1)$$

where Y_{igt} represents the outcome variable for individual *i* in age group g at time *t*, $Treatment_g$ is an indicator equal to one if the individual belongs to the treatment group, $Post_t$ is an indicator equal to one if the time period is after January 2006, $\boldsymbol{X}_{igt}$ is a vector of demographic control variables (including sex, race/ethnicity, educational attainment, marital status, household income, and Census region), η_g is a vector of age-fixed effects, and ϑ_t is a vector of year-fixed effects. Standard errors are clustered at the individual level, and all analyses include HRS survey weights.

The coefficient β represents the causal effect of prescription drug coverage provided that the control group is a good counterfactual for the treatment group, i.e. in the absence of Part D,

both groups would have trended similarly. In order to assess the plausibility of this assumption, I conduct tests for parallel trends using an event study approach. I estimate a model similar to Equation 1, but replace the $Treatment_g * Post_t$ interaction term with a vector of interactions for each year indicator and $Treatment_g$, excluding the year just prior to 2006 as the base year. I conduct an F-test to test whether all the pre-2006 interaction terms are jointly equal to 0. A p-value greater than 0.10 means failure to reject the null hypothesis of equal trends between the treatment and control groups before 2006, which increases our confidence in interpreting β as a causal effect.

2.4.2 Caregiving Outcomes

I use the ATUS data and a similar DD model to evaluate the impact of Part D on non-elderly adults' time spent caring for and helping household adults. For this set of outcomes, the sample is restricted to adults aged 27 to 64. The treatment group is defined as those who have at least one elderly person (age 65 or older) in their household, i.e. those whose household members are eligible for Part D coverage. The control group consists of those whose household members are all below age 65, i.e. those whose household members were not affected by the introduction of Part D.[27] The baseline model is:

$$Y_{igt} = \alpha + \beta\left(Treatment_g * Post_t\right) + \eta Treatment_g + \gamma \boldsymbol{X}_{igt} + \vartheta_t + \varepsilon_{igt} \quad (2)$$

where Y_{igt} represents the outcome variable for individual *i* in treatment group g at time *t*, $Treatment_g$ is an indicator equal to one if the individual belongs to the treatment group, $Post_t$ is an indicator equal to one if the time period is after January 2006, $\boldsymbol{X}_{igt}$ is a vector of

[27] Ideally, I would also like to include in my treatment group individuals who have elderly parents or other relatives who do not live with them. However, the ATUS only provides ages of parents and other relatives if they live in the same household as the respondent.

demographic control variables (including age, sex, race/ethnicity, educational attainment, marital status, household size, Census region, and an indicator for whether the interview took place on a weekend or holiday), and ϑ_t is a vector of year-fixed effects. Standard errors are robust, and all analyses include ATUS survey weights.

2.5 Results

I first examine the impact of Part D on functional outcomes for elderly adults. These results are displayed in Table 2-2. I find that Part D reduced the probability of having any ADL/IADL limitations for adults aged 65-74 by 1.6 percentage points (a 9 percent decline from pre-2006 levels) and the number of ADL/IADL limitations by 0.06 limitations (a 12 percent reduction from baseline). Next, I examine ADLs and IADLs separately. Although there are significant improvements for both types, Part D is associated with larger declines in IADL limitations (14 percent decrease in the probability of any IADL limitations versus an 8 percent decline in the probability of ADL limitations). Table 2-2 also displays results that stratify the sample by marital status and sex. I find larger improvements in IADL outcomes for married individuals and for men.

In Appendix Table 2- 4, I analyze changes in the 10 specific ADLs and IADLs. Part D coverage has the largest effect on reducing limitations in bathing/showering, eating, grocery shopping, managing money, using a telephone, and taking medications. Appendix Table 2- 5 shows DD estimates for the impact of Part D on outcomes other than functional outcomes. I confirm earlier studies' findings that Part D improves self-assessed health and increases regular use of prescription drugs. I also find that Part D increases the probability of having any doctor visit in the past two years, suggesting that doctor visits may be a potential mechanism for

increasing prescription drug utilization. However, there is no effect on gross and fine motor limitations, number of doctor visits, or home health care visits.

Next, I present in Table 2-3 the effects of Part D on non-elderly adults' time spent caregiving for household adults. I find that Part D reduces the probability of spending any time caregiving by 3.5 percentage points for women (a 22 percent decline from pre-2006 levels). However, there is no change in caregiver status among men. The overall effect of Part D on hours per week spent caregiving decrease by a statistically insignificant) 0.2 hours. Women's caregiving hours reduce by 0.6 hours per week, but men's caregiving hours actually increase by 0.4 hours per week. This suggests that Part D may change distribution of caregiving responsibilities within families.

Appendix Table 2- 8 shows that the reduction in caregiving for women were driven by activities related to providing physical care for household adults and providing medical care to household adults. Increases in male caregiving were driven by providing physical care for household adults and obtaining medical and care services for household adults. Appendix Table 2- 9 presents interesting heterogeneity in caregiving outcomes. I find that the largest declines in caregiving occur for women who are in the labor force, do not have children, are aged 55 to 64, are White, have high school education, and are married.

Finally, I explore whether this reduction in caregiving is accompanied by an increase in labor force participation. Table 2-4 presents DD estimates for the impact of Part D on labor market outcomes for non-elderly potential caregivers. In spite of its impact on caregiving, there is little evidence that Part D increases labor force participation or employment. In fact, Part D is associated with a small increase in the probability of being unemployed among non-elderly men with elderly members of their household. There are no detectable effects of the policy on other

employment outcomes, such as full-time versus part-time employment, self-employment, working without pay, or usual hours worked (Appendix Table 2- 10). I find similar results using data from the HRS (Appendix Table 2- 13) and the CPS (Appendix Table 2- 14).

Since I find reductions in caregiving among older women, I explore possible activities that women substituted for caregiving after the implementation of Part D. In Appendix Table 2-12, I present estimates for the effect of the policy on older women's time spent on a comprehensive set of activities reported in the ATUS. While Part D does not impact time spent on work and employment, there were substantial increases in time spent on housework and providing care for non-household children. These findings suggest that when the elderly become eligible for prescription drug coverage, caregivers spend less time caregiving and substitute this extra time with household production activities.

2.5.1 Parallel Trends Tests and Sensitivity Analyses

To assess pre-policy parallel trends between the treatment and control groups, a key identifying assumption of the DD model, I first visually examine trends for the treatment and control groups for functional outcomes (Figure 2-1), caregiving outcomes (Figure 2-2), and labor market outcomes (Figure 2-3). I provide further evidence that the treatment and control groups followed similar trends before 2006 by estimating event study models. Results are reported in Appendix Figure 2- 3, Appendix Figure 2- 4, and Appendix Figure 2- 5. For most outcomes, event study regressions show little evidence of significant differences in trends between the treatment and control groups before 2006.

I conduct several checks to assess the robustness of my findings. For my baseline models of functional outcomes, I restrict my sample to individuals aged 55 to 74. Appendix Table 2- 6 shows that results are not sensitive to this sample restriction; results are remarkably stable, even

if I reduce the age band by one year on each side. I also show DD estimates from a number of sensitivity analyses in Appendix Table 2- 7 and Appendix Table 2- 11: including a right-hand-side variable that controls for region/state-level Medicare Advantage penetration rates, using robust standard errors rather than individual-clustered, excluding individuals aged 63-64, excluding the year 2004, excluding years 2010 and later, omitting demographic control variables from the right hand side, and including individual fixed effects. For the most part, the key results are remarkably similar to those from the baseline model.

2.6 Discussion

This study finds a statistically significant and economically meaningful decline in ADL and IADL limitations due to the introduction of Medicare Part D, suggesting that increased access to pharmaceuticals improves functional outcomes among older adults. I also find that Part D reduced time spent caregiving among non-elderly household members, particularly women. Thus, the findings of this study imply that the impact of policies that reduce drug prices and expand prescription drug insurance extend beyond improving health outcomes for their targeted elderly populations; they also have positive spillover effects for non-elderly potential caregivers.

My DD estimates suggest that Part D decreased the probability of having any ADL and IADL limitations by 2 percentage points (or 9 percent compared to pre-2006 levels) and reduced the number of hours non-elderly women spend providing care to household adults by 0.6 hours per week (or 42 percent). Taken together, these effects imply that for every one elderly adult who no longer has functional limitations, there is a 30-hour per week reduction in the number of caregiving hours by a non-elderly female caregiver.

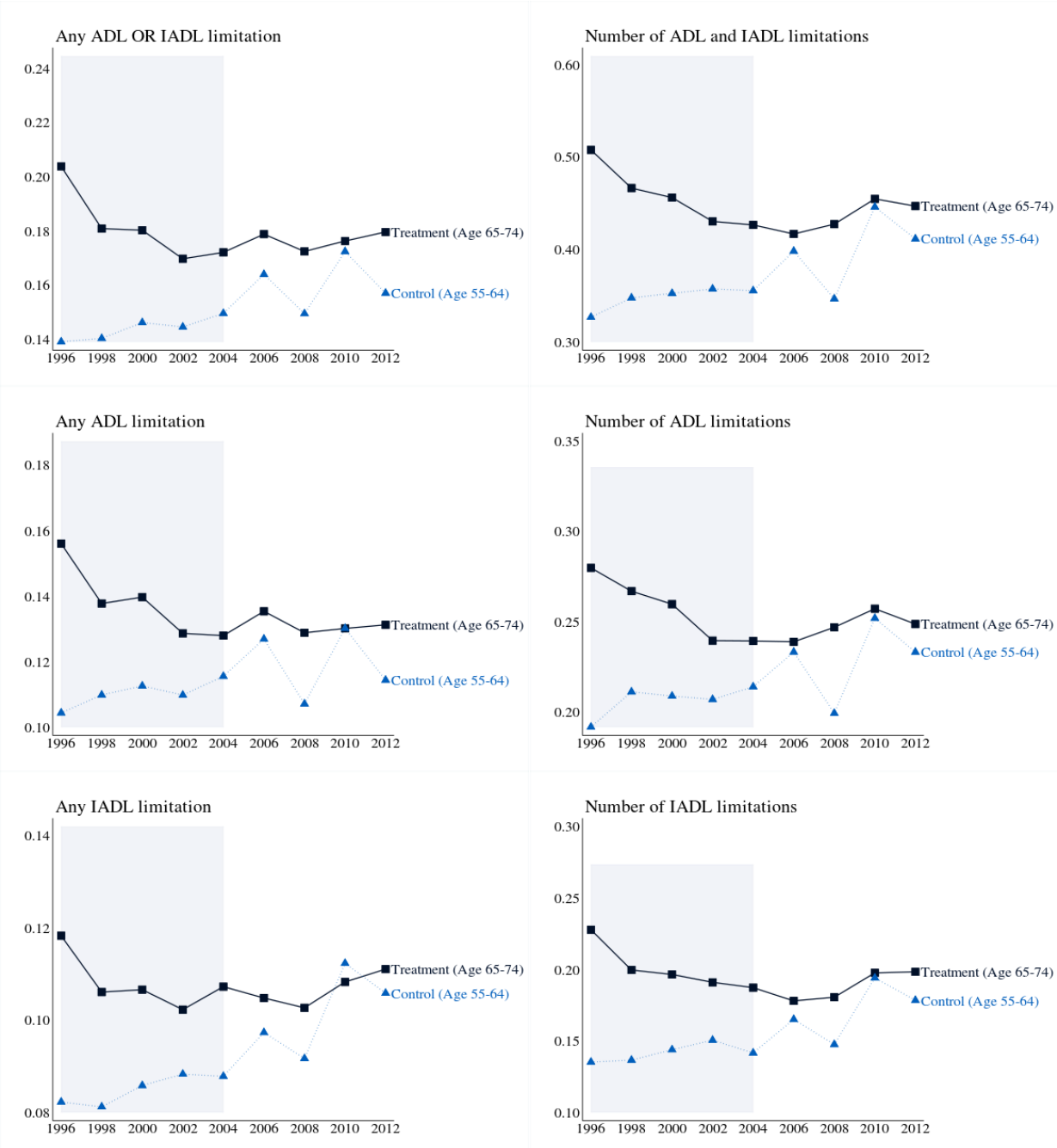
This analysis has several limitations. First, since over two-thirds of elderly people had prescription drug coverage even before the implementation of Part D, there is general concern

that unobserved beneficiary characteristics may help explain estimated effects. If it were true that unhealthy individuals were more likely to seek prescription drug coverage before 2006, then my estimates of the effect of Part D on functional outcomes would be a lower bound of the true effect of prescription drug coverage on functional outcomes. Second, the ATUS data identifies ages of parents, spouses, and other relatives only if these individuals live in the same household as the respondent. This means that members of my control group (identified as those without any *household* members over age 65) may have in fact been affected by Part D if their non-household parents/relatives became eligible for the policy. Finally, enrollment in Medicare Advantage grew substantially from 2005 to 2013; it is possible that my DD estimates are in fact picking up the effect of Medicare Advantage growth. To alleviate this concern, I conduct a robustness check in which I include region/state-level Medicare Advantage penetration rates on the right-hand side.

In spite of these caveats, this study makes several important contributions to the literature. First, to my knowledge, no other study has used methods of causal inference to evaluate the impact of Medicare Part D on activities of daily living. This paper thus adds to the scant evidence that prescription drug coverage improves health outcomes for elderly people. Moreover, we know little about how Part D affected the informal long-term care needs of elderly adults. I provide some of the first evidence that increasing pharmaceutical access for the elderly not only improves their own functional outcomes, but also has positive spillover effects for non-elderly caregivers.

Figures

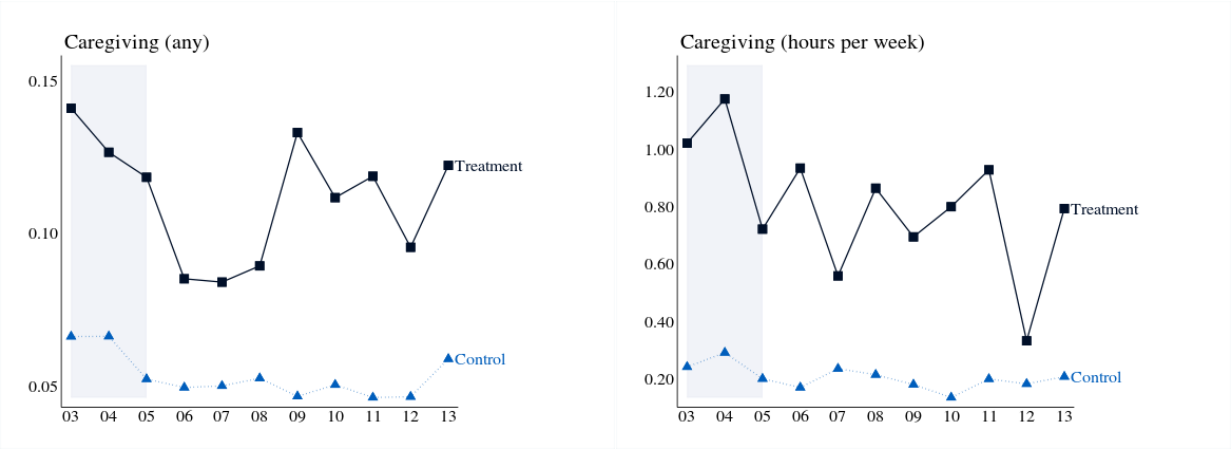
Figure 2-1. Trends in ADL & IADL Limitations



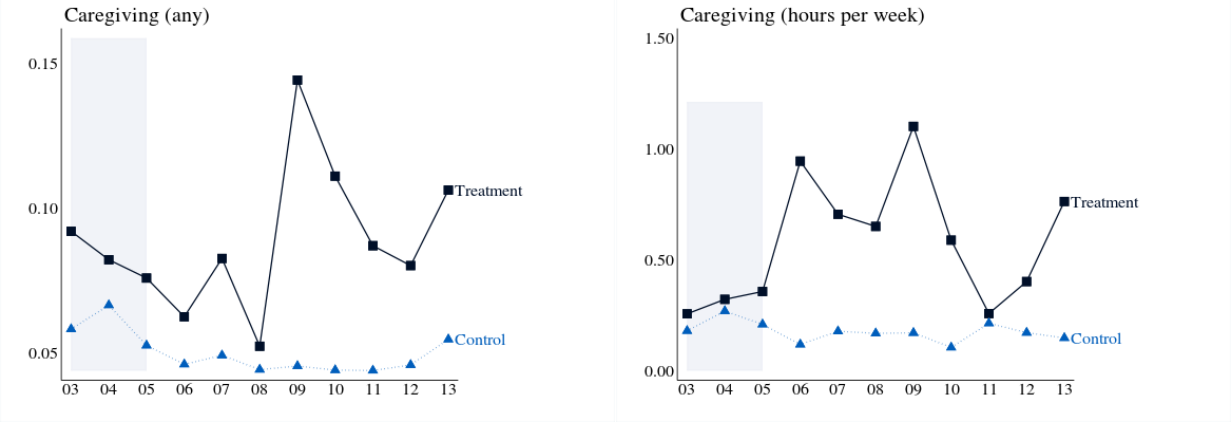
Source: Author's calculations based on HRS 1996-2012 (N=103,755). Sample is restricted to adults age 55-64. Estimates include HRS sampling weights.

Figure 2-2. Trends in Non-Elderly Adults' Time Spent Caregiving for Household Adults

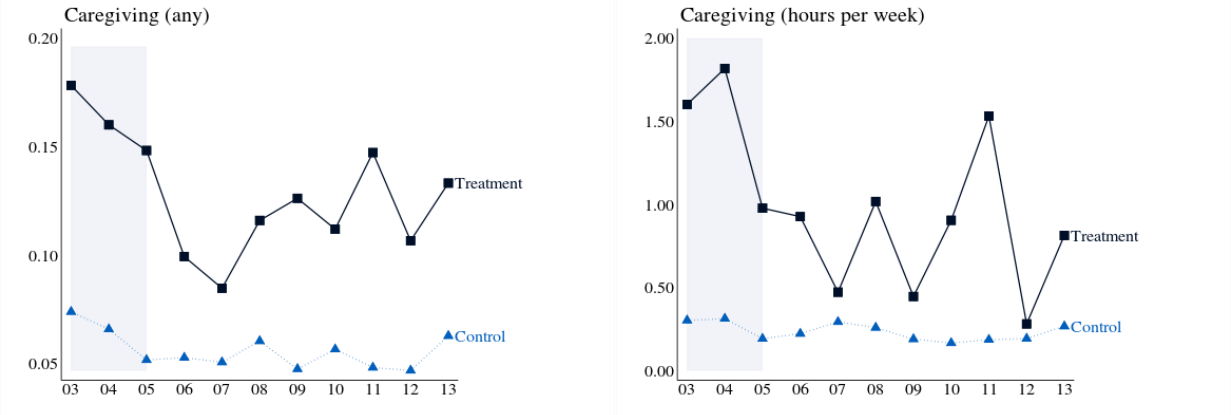
Panel A: Pooled



Panel B: Men



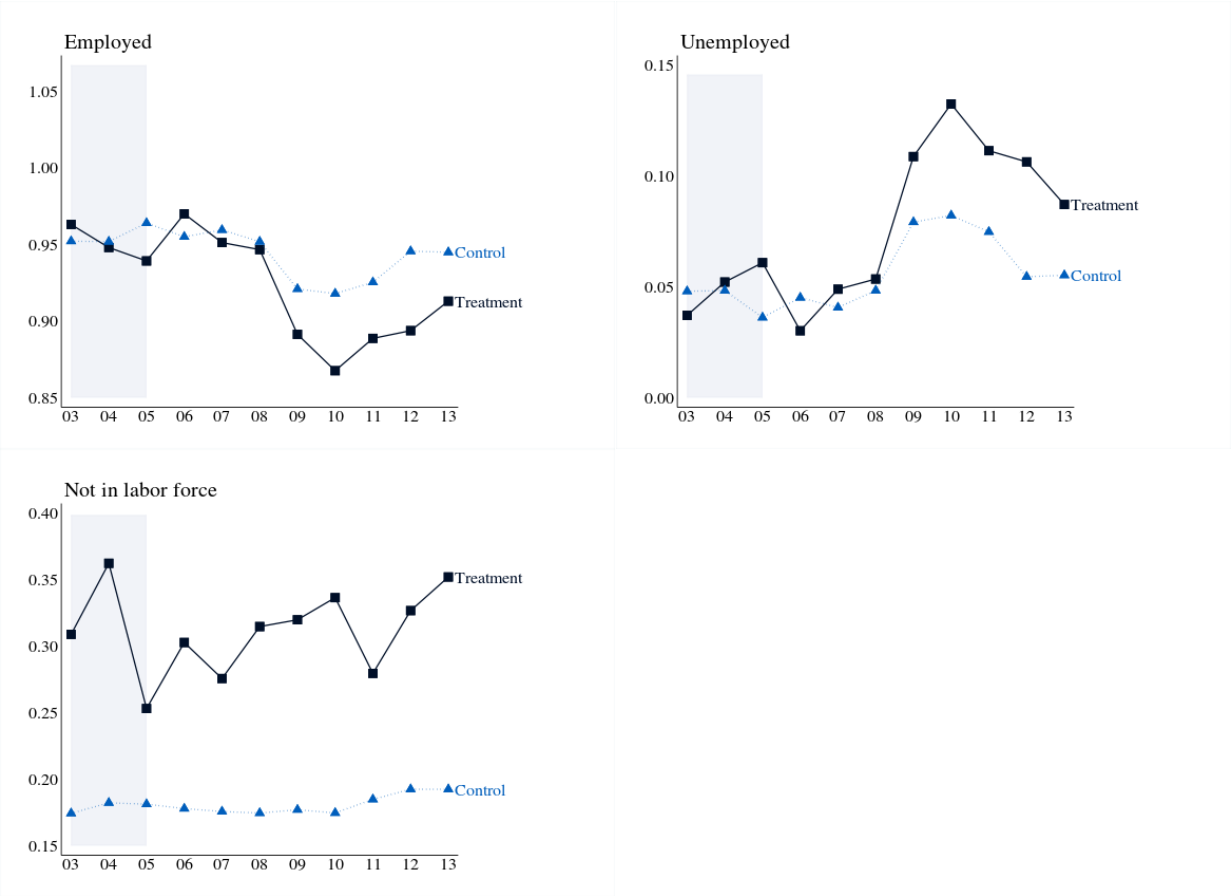
Panel C: Women



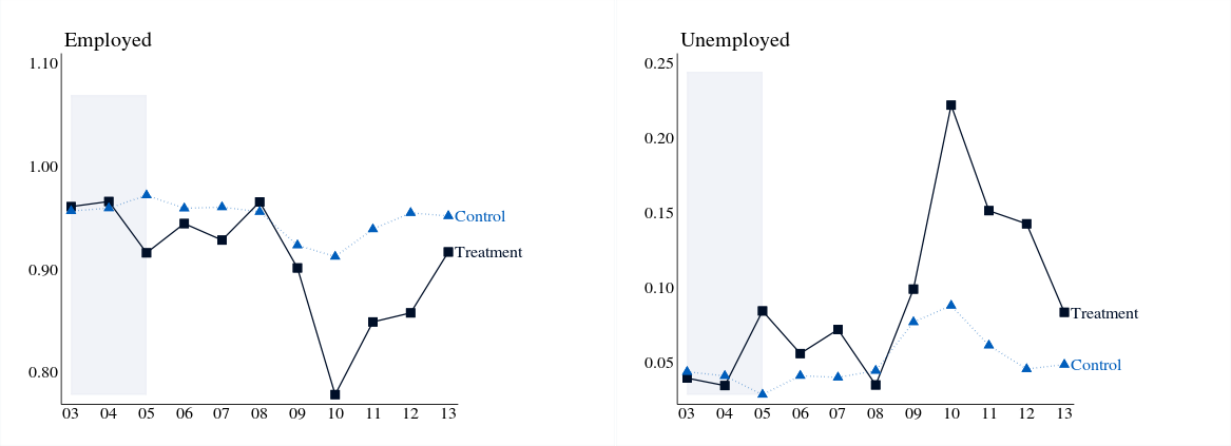
Source: Author's calculations based on ATUS 2003-13 (N=101,423). Sample is restricted to adults age 27-64. The treatment group comprises individuals who have at least one elderly adult in their household. The control group comprises individuals who have no elderly adults in their household. Estimates include ATUS sampling weights.

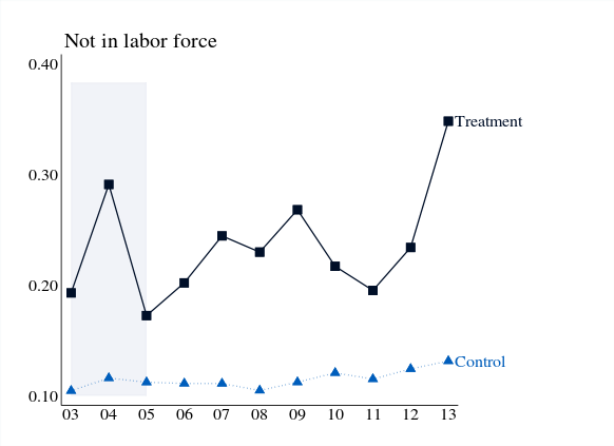
Figure 2-3. Trends in Non-Elderly Adults' Labor Force Outcomes

Panel A: Pooled

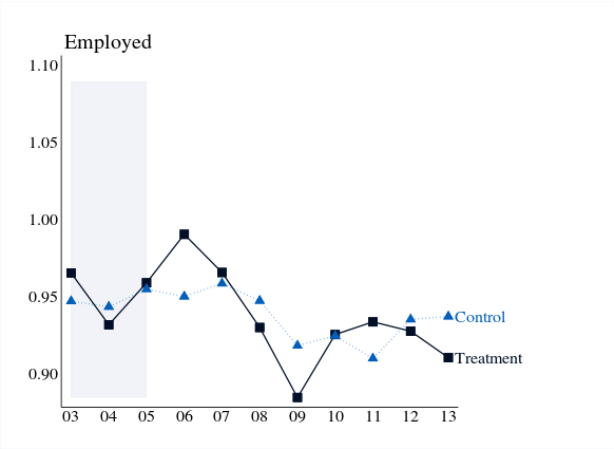


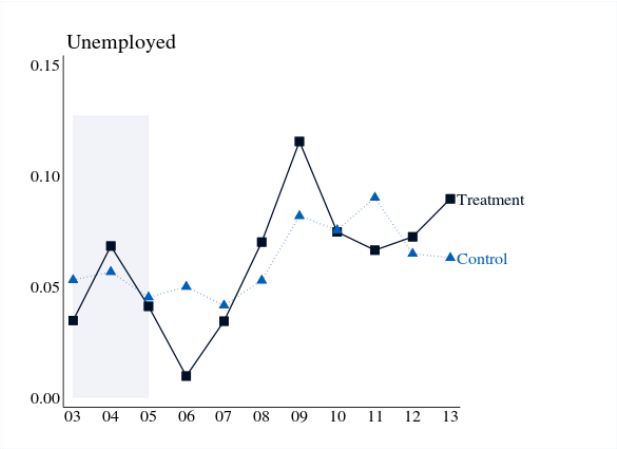
Panel B: Men

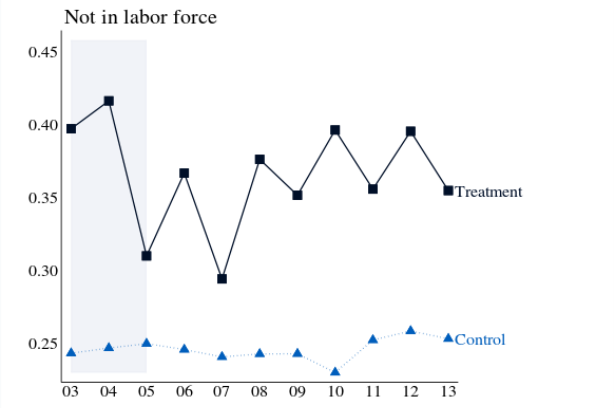




Panel C: Women







Source: Author's calculations based on ATUS 2003-13 (N=101,423). Sample is restricted to adults age 27-64. The treatment group comprises individuals who have at least one elderly adult in their household. The control group comprises individuals who have no elderly adults in their household. Estimates include ATUS sampling weights.

Tables

Table 2-1. Prevalence of ADL & IADL Limitations in the HRS

	ADL or IADL Limitations		ADL Limitations		IADL Limitations	
	Any	Number [0-10]	Any	Number [0-5]	Any	Number [0-5]
Pooled (age 50+)	0.202	0.6	0.150	0.3	0.132	0.3
By age						
50-54	0.135	0.3	0.099	0.2	0.087	0.2
55-59	0.148	0.4	0.110	0.2	0.092	0.2
60-64	0.159	0.4	0.122	0.2	0.097	0.2
65-69	0.163	0.4	0.123	0.2	0.096	0.2
70-74	0.194	0.5	0.145	0.3	0.119	0.2
75-79	0.261	0.7	0.190	0.4	0.169	0.3
80-84	0.352	1.0	0.253	0.5	0.247	0.5
85+	0.524	1.9	0.390	0.9	0.414	1.0
By sex						
Male	0.183	0.5	0.131	0.2	0.113	0.2
Female	0.218	0.6	0.166	0.3	0.148	0.3
By household income						
Quartile 1	0.386	1.2	0.297	0.6	0.275	0.6
Quartile 2	0.237	0.6	0.173	0.3	0.155	0.3
Quartile 3	0.152	0.4	0.111	0.2	0.091	0.2
Quartile 4	0.084	0.2	0.059	0.1	0.047	0.1
By educational attainment						
Less than high school	0.350	1.1	0.263	0.6	0.248	0.5
High school	0.195	0.5	0.145	0.3	0.124	0.2
Some college	0.166	0.4	0.122	0.2	0.104	0.2
College or more	0.116	0.3	0.085	0.2	0.069	0.1
By race/ethnicity						
White, non-Hispanic	0.185	0.5	0.135	0.3	0.119	0.2
Black, non-Hispanic	0.291	0.9	0.225	0.5	0.202	0.4
Other, non-Hispanic	0.227	0.6	0.166	0.3	0.155	0.3
Hispanic	0.273	0.9	0.216	0.5	0.178	0.4
By marital						

status						
Married	0.157	0.4	0.114	0.2	0.098	0.2
Unmarried	0.278	0.8	0.211	0.4	0.190	0.4
By veteran status						
Veteran	0.186	0.5	0.134	0.3	0.113	0.2
Non-veteran	0.207	0.6	0.155	0.3	0.138	0.3

Source: Author's calculations based on HRS 1996 to 2012 (N=103,755). Estimates include HRS sampling weights. ADL limitations are bathing, dressing, eating, getting in and out of bed, and walking across the room, and IADL limitations are talking on the telephone, managing money, taking medications, shopping for groceries, and preparing meals.

Table 2-2. DD Estimates for Impact of Part D on ADL & IADL Limitations for the Elderly

	Pooled	Married	Not married	Men	Women
Any ADL/IADL limitations	-0.016**	-0.017**	-0.013	-0.016	-0.016*
	(0.007)	(0.007)	(0.013)	(0.010)	(0.009)
	[μ=0.178]	[μ=0.153]	[μ=0.224]	[μ=0.167]	[μ=0.187]
Number ADL/IADL limitations [0-10]	-0.056**	-0.070***	-0.026	-0.073**	-0.045
	(0.023)	(0.024)	(0.048)	(0.032)	(0.032)
	[μ=0.450]	[μ=0.382]	[μ=0.580]	[μ=0.408]	[μ=0.484]
Any ADL Limitations	-0.011*	-0.012*	-0.009	-0.012	-0.011
	(0.006)	(0.006)	(0.012)	(0.009)	(0.008)
	[μ=0.135]	[μ=0.115]	[μ=0.174]	[μ=0.119]	[μ=0.149]
Number ADL limitations [0-5]	-0.023*	-0.029**	-0.011	-0.036*	-0.014
	(0.014)	(0.014)	(0.029)	(0.019)	(0.020)
	[μ=0.253]	[μ=0.214]	[μ=0.329]	[μ=0.222]	[μ=0.280]
Any IADL limitations	-0.015***	-0.017***	-0.010	-0.013*	-0.018**
	(0.005)	(0.006)	(0.011)	(0.007)	(0.007)
	[μ=0.107]	[μ=0.090]	[μ=0.139]	[μ=0.099]	[μ=0.113]
Number IADL limitations [0-5]	-0.032***	-0.040***	-0.015	-0.036**	-0.031**
	(0.011)	(0.012)	(0.023)	(0.016)	(0.015)
	[μ=0.196]	[μ=0.168]	[μ=0.251]	[μ=0.186]	[μ=0.205]
Observations	103,755	70,190	33,565	45,659	58,096

Source: Author's calculations based on Health and Retirement Study, 1996-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for respondent's household income, sex, race/ethnicity, educational attainment, region of residence, and marital status; age fixed effects; and year fixed effects. Estimates include HRS sampling weights. Individual-clustered standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2-3. DD Estimates for Impact of Household's Part D Eligibility on Non-Elderly Adults' Time Spent Caregiving

	Pooled	Men	Women
Caregiving (any)	-0.013	0.017	-0.035*
	(0.013)	(0.017)	(0.018)
	[μ=0.129]	[μ=0.083]	[μ=0.162]
Caregiving (hours per week)	-0.185	0.383**	-0.602**
	(0.166)	(0.165)	(0.261)
	[μ=0.963]	[μ=0.311]	[μ=1.444]
Observations	101,423	45,193	56,230

Source: Author's calculations based on American Time Use Survey, 2003-2013. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights. Robust standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2-4. DD Estimates for Impact of Household's Part D Eligibility on Non-Elderly Adults' Labor Force Outcomes (ATUS)

	Pooled	Men	Women
Employed	-0.021*	-0.040**	-0.007
	(0.011)	(0.018)	(0.013)
	[μ=0.950]	[μ=0.946]	[μ=0.953]
Unemployed	0.021*	0.040**	0.007
	(0.011)	(0.018)	(0.013)
	[μ=0.050]	[μ=0.054]	[μ=0.047]
Observations	81,673	39,715	41,958
Not in labor force	0.003	0.016	-0.004
	(0.016)	(0.024)	(0.022)
	[μ=0.306]	[μ=0.217]	[μ=0.371]
Observations	101,423	45,193	56,230

Source: Author's calculations based on American Time Use Survey, 2003-2013. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights. Robust standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3 Drugs and Diet: The Impact of Prescription Drug Coverage on Food Habits

Abstract

Incentivizing nutritious diet and other healthy behaviors is therefore a key public health policy in the United States, as poor diet is associated with chronic illness, including obesity, diabetes, and heart disease, which are costly for the health care system in the long. However policies that are intended to improve health outcomes, such as expansion of prescription drug coverage, may unintentionally worsen individuals' diets by reducing their costs of eating poorly. In this study, I examine how elderly adults' dietary habits respond to increased prescription drug coverage after the implementation of Medicare Part D in 2006. I use nationally-representative survey data to compare changes in diet among elderly versus near-elderly people after the implementation of the policy in 2006. I find strong and consistent evidence that Part D reduced healthy eating among seniors. These results are reflective of ex-ante moral hazard and suggest that poor eating habits may be an unintended spillover effect of increased prescription drug access.

3.1 Introduction

Prescription drugs represent one of the most rapidly growing components of US health care spending, increasing from $354 billion in 2009 to $457 billion in 2015 (Assistant Secretary for Planning and Evaluation, 2016). Prescription medication utilization is particularly high among enrollees in public insurance programs; Medicare and Medicaid paid $219.3 billion or nearly half of the $457 billion spent on prescription drugs in 2015 (Centers for Medicare & Medicaid Services, 2016). While rapid innovation in pharmaceuticals has contributed to longevity and quality of life (Lichtenberg, 2007; Musini, Tejani, Bassett, & Wright, 2009) and reduced hospitalizations and non-drug medical spending (Afendulis et al., 2011; Kaestner et al., 2017), improved access to drugs may have unintended negative spillover effects on individuals' health behaviors. For example, if a person wants to lower their blood cholesterol levels, they can do so either by taking prescription statins or by modifying their diet, particularly reducing their intake of saturated fat and transfats (American Heart Association, 2017). If people view prescriptions and healthy diet as substitutes, then reducing prescription prices through expansions of prescription drug insurance may inadvertently lead to worse diets and higher utilization of prescription drugs.

Poor diet is associated with a number of chronic illnesses, including obesity, diabetes, and heart disease. These diseases are costly for the health care system in the long run. Many of the costs associated with these diseases are not borne by the individual patient but rather by society at large in the form of payments from public health insurers, informal caregiving from friends and family members, and lost economic productivity. One study finds that the social cost of obesity alone exceeds $186 billion per year (Cawley & Meyerhoefer, 2012). Since individuals do not take into account these external costs when deciding to engage in risky behaviors,

government intervention may be needed to align the private costs of risky behaviors with the social costs. Incentivizing nutritious diet and other healthy behaviors is therefore a key public health policy in the United States (US Department of Health and Human Services, 2014a).

Several existing state and federal policies attempt to modify individuals' diets by increasing prices of unhealthy products (for example, soda taxes and candy taxes) and removing information asymmetries (for example, requiring labeling of nutrition facts). However, in some cases, government programs which are intended to improve health may unintentionally reduce individuals' private costs of engaging in risky behaviors. One such example is the provision of publicly-subsidized prescription drug coverage for elderly people through the introduction of Medicare Part D in January 2006. Part D reduced the fraction of drug uninsured elderly from 24 percent to 7 percent in its first year[28] and substantially increased their drug utilization by between 6 and 13 percent (Ketcham & Simon, 2008; Levy & Weir, 2009; Lichtenberg & Sun, 2007; Yin et al., 2008).[29] Lipid regulators and non-insulin diabetes drugs were among the top five therapeutic classes of drug prescribed under Part D in 2006-07 (Engelhardt & Gruber, 2011).

While Part D played an important role in improving health and financial outcomes for the elderly (Ayyagari, 2016; Ayyagari & Shane, 2015; Chen et al., 2018), recent evidence points to unintentional harmful effects of the policy on health behaviors (Asfaw, 2019). Asfaw finds that the introduction of Part D reduced moderate exercise at the extensive and intensive margins and increased the probability of being overweight. However, we know little about how individuals substitute between prescription drug and diet. This paper examines the impact of prescription

[28] Even among those seniors who had drug coverage before 2006, most had less generous coverage with annual caps on spending (Gold, 2001). Past studies found that such spending caps were associated with reduced drug adherence among seniors (Hsu et al., 2006).

[29] Most of these studies measured "intent to treat" effects, and nearly two-thirds of the elderly had prescription drug insurance prior to Part D, so they imply much larger impacts for the sample who actually went from uninsured to insured.

drug coverage on elderly adults' food habits, using the exogenous increase in prescription drug coverage for the over-65 population after the implementation of Part D. I use t nationally-representative survey data to compare changes in diet among elderly versus near-elderly people after the implementation of the policy in 2006.

I find strong and consistent evidence of substitution between prescription drugs and healthy diet. This is concerning because compared to diet, prescription drugs pose larger financial costs for public payers, can lead to adverse side-effects for patients (such as cerebral hemorrhage, renal failure, and rhabdomyolysis from statins) (Dimmitt, Stampfer, & Martin, 2017), and have lower rates of efficacy than most people believe (Greger, 2018).[30] The consensus among most physicians and researchers is that diet and drugs are not mutually exclusive and that a healthy diet should accompany drug utilization. Thus, the finding that drug utilization is accompanied with poorer diet is a major public health concern. As policymakers discuss ways to make prescription drug more accessible to individuals, they should also implement supplemental policies to promote healthy eating to address potential negative spillover effects of increased drug utilization.

3.1.1 Previous Literature on Ex-Ante Moral Hazard in Health Insurance

This paper draws on the broader literature of ex-ante moral hazard in health insurance. Ex-ante moral hazard, which refers to low investments in self-protective activities in the presence of market insurance, was first modeled in Ehrlich and Becker (1972); this seminal paper discussed the tradeoffs between self-insurance, self-protection, and market insurance. In an

[30] For example, Greger (2018) reports that statins offer at most a 3 percent absolute risk reduction for subsequent heart attack or death over six years, whereas a heart-healthy diet may offer an absolute risk reduction of 60 percent after fewer than four years. Nevertheless, survey results indicate that patients believe statins are about 100 times more effective than they actually are in preventing heart attacks.

application of the Ehrlich and Becker model to health care, an agent can protect herself against risk by 1) purchasing health insurance, 2) self-insuring via intertemporal savings, or 3) investing in self-protective activities (such as preventive care and healthy behaviors) to reduce the probability of sickness. Without insurance, agents engage in an optimal combination of self-insurance and protection, but if the agent purchases health insurance, then she no longer bears the full cost of falling sick. Thus, the marginal benefit of self-protection falls, and consequently the agent's optimal level of self-protection decreases.

Findings from the existing empirical literature on the existence of ex-ante moral hazard in health insurance are mixed. (Appendix 3-A provides a comprehensive review of this literature.) There is some evidence that Medicare decreases physical activity and increases smoking and drinking among elderly men, while it has no detectable effect on risky behaviors among women (D. Dave & Kaestner, 2009; de Preux, 2011). Others study younger populations and find that providing health insurance to young adults increases risky drinking (Barbaresco, Courtemanche, & Qi, 2015), though other studies using different data find no evidence of changes in smoking, drinking or drug utilization (Breslau et al., 2017; J. Lee, 2018). Results are also mixed for low-income populations: some find that health insurance expansions for the poor increase risky drinking (C. Courtemanche, Marton, Ukert, Yelowitz, & Zapata, 2019) and smoking during pregnancy (D. M. Dave, Kaestner, & Wehby, 2019); others find no detectable effect on exercise, smoking, drinking, or BMI (Cawley, Soni, & Simon, 2018; C. Courtemanche, Marton, Ukert, Yelowitz, & Zapata, 2018; K. Simon, Soni, & Cawley, 2017); and still others actually find improvements in health behaviors for this population, such as reduced BMI and decreased consumption of cigarettes, alcohol, and soda (Cotti, Nesson, & Tefft, 2019; C. J. Courtemanche & Zapata, 2014; He, Lopez, & Boehm, 2018). Two randomized controlled trials find no impact

of health insurance on smoking and mixed results for body weight (Baicker et al., 2013; Bhattacharya, Bundorf, Pace, & Sood, 2011; Brook et al., 1983). Another study exploiting state laws that require plans to cover diabetes treatment find that more generous coverage does increase BMI (Klick & Stratmann, 2007). Researchers have also studied ex-ante moral hazard in non-US settings. There is evidence that insurance improves healthy behaviors in Britain (Courbage & de Coulon, 2004), but reduces the probability of engaging in healthy behaviors in China and Ghana (Qin & Lu, 2014; Yilma, Van Kempen, & De Hoop, 2012).

One potential reason that previous studies find mixed results may be that health insurance provides incomplete protection against the costs of disease. While insurance may cover the monetary costs associated with illness, it does not cover the non-financial costs such as pain and suffering. Moreover, the medical procedures covered by non-drug insurance (such as surgery) may impose utility loss above and beyond the financial cost. One study finds that 88 percent of patients going for surgery experienced preoperative fear; the top three causes of their anxieties are fear of postoperative pain, intraoperative awareness, and postoperative drowsiness (Ruhaiyem et al., 2016). Fear and anxiety associated with medical procedures may be a reason why individuals resist changing their behaviors, even after gaining health insurance.

Notably, the majority of these existing studies explore ex-ante moral hazard in the context of comprehensive insurance coverage. Prescription drugs are widely used and not associated with the same levels of fear and anxiety as invasive medical procedures. One would imagine, therefore, that there should be a larger behavioral response to prescription drug coverage than non-drug coverage. To my knowledge, only one paper looks at the impacts of prescription drug coverage isolated from other health insurance coverage. Asfaw (2019) studies the introduction of Part D and finds that the policy decreased vigorous physical activity and

increased the probability of being overweight. While he does not find an impact on smoking and other types of physical activities, his findings suggest that prescription drug coverage may indeed be associated with higher levels of ex-ante moral hazard.

This paper makes two primary contributions to this literature. First, to my knowledge, this is the first paper to study the effect of prescription drug coverage on dietary outcomes. Part D is a promising context in which to study the effects of prescription drug coverage, given its substantial impact on out-of-pocket (OOP) drug prices and utilization. (See Appendix 2-A for a review of papers studying the impact of Part D on drug utilization.) Second, this paper is more generally informative about the presence of ex-ante moral hazard in the context of prescription drug insurance. Ex-ante moral hazard causes people to invest insufficiently in self-protection, and it is thus important for policymakers to be aware of this unintended side effect of expanding prescription drug coverage.

3.1.2 Conceptual Framework: How Part D May Impact Diet

Part D could theoretically affect elderly individuals' diets in several ways. First, increased access to prescription drugs may cause ex-ante moral hazard; people have less incentive to undertake health-promoting behaviors because they no longer bear the full financial cost of their future illness. On the other hand, drug coverage leads to more physician visits to obtain prescriptions. During these visits, doctors may advise patients to improve their diets. Finally, Part D increased seniors' disposable income, and they may have allocated these funds to improving their diets.[31]

[31] Existing studies show that before Part D, prescription drugs produced high financial burdens for elderly people. In a 2003 survey of noninstitutionalized Medicare beneficiaries above age 65, 90% of respondents reported taking prescription drugs, predominantly pills, inhalers, creams, and eyedrops. However, only 75% of all respondents and 66% of low-income respondents had prescription drug coverage, and so out-of-pocket prescription costs were high. Nearly one-third of respondents spent more than $100 per month, and five percent reported buying

These three mechanisms work in opposing directions. For example, assume that there are two methods for someone to prevent a heart attack: 1) reduce consumption of unhealthy foods high in cholesterol and saturated fats, or 2) take a statin. If the patient derives utility from eating unhealthy foods, then "abstaining" is costly for her. When statins are subsidized through prescription drug insurance, she would be less likely to abstain and so we would expect an increase in cholesterol and fat consumption. On the other hand, when the patient goes to the doctor to obtain her statin prescription, the doctor may inform her of the negative effects unhealthy eating has on her condition; this information effect may convince the patient to improve her diet. Finally, unhealthy foods tend to be inferior goods, so when the patient's disposable income increases through prescription drug insurance (due to less out of pocket spending on statins and other drugs), she would buy less unhealthy food. The net effect of prescription drug insurance on diet is thus ambiguous and ultimately an empirical question.

3.2 An Economic Model of Ex-Ante Moral Hazard

This section develops a model that predicts how Medicare Part D changes its beneficiaries' incentives; the model is based on one developed in a seminal paper on ex-ante moral hazard (Ehrlich & Becker, 1972), but my model differs in the following ways. In the Ehrlich and Becker model, the main risk agents face is foregone wages, but the population I model is above the age of 65, so foregone income is less salient; instead, the main risk in my model is out-of-pocket medical spending. Also, Ehrlich and Becker include an endogenous

prescriptions from Canada or Mexico. Twenty-five percent reported forgoing prescription drugs in the past year because of cost. Most respondents who had prescription drug coverage obtained coverage through employer plans or privately purchased plans. Those with privately purchased coverage had higher spending and cost-related nonadherence than those with employer-sponsored coverage and Medicaid (Safran et al., 2005).

variable to indicate the purchase of insurance, whereas in my model, the only endogenous variable is behavior.

The agent in this model is a Medicare-eligible person who is above the age of 65. The agent chooses the amount of a healthy behavior to maximize expected utility. There are two states of the world depending on the agent's health status: *Sick* and *Healthy,* and each state has an associated utility based on the agent's net income in that state. I assume that labor supply is zero in both states as the agent is above age 65 and likely to be retired. The agent receives retirement income *Y* which is independent of health status and exogenous in this model.

Health status depends on probabilities over which individuals have at least some level of control. Agents can undertake a healthy behavior, i.e. consume a nutritious diet, which would reduce their probability of getting sick (p). Healthy behavior is denoted as B and measured in time-units. Mathematically, $p(B)$ is a decreasing convex function with $\frac{dp}{dB} < 0$ and $\frac{d^2p}{dB^2} > 0$ (See Figure 3-1). The reason for the convexity assumption is that presumably going from zero units of nutrition to one unit of nutrition would result in major health benefits; however, the marginal benefit of each additional unit of nutrition would naturally fall as the agent starts to eat more and more nutritiously.

Undertaking the healthy behavior also imposes some cost on the agent, which I quantify as wB, where w represents the agent's shadow price of time. In the event of sickness, agents face an exogenous financial loss L, which is equivalent to the medical cost of treating sickness.

Agents are trying to maximize expected utility, which is calculated as the sum of utility when healthy and utility when sick, each weighted by their respective probability of occurring.

$$max\ EU\ (B) = \ p(B) \cdot u(y^s) + [1 - p(B)] \cdot u(y^h)$$

Equation 3-1

where the income when sick is $y^s = Y - L - wB$ and income when healthy is $y^h = Y - wB$.[32] I assume that *u(y)* and *w* are strictly positive, while *B* is greater than or equal to 0. The only endogenous decision variable in the model is *B*. Taking the derivate of Equation 3-1 with respect to choice variable *B* results in the first order condition:

$$p(B^*) \cdot \frac{du}{dy^s} \cdot (-w) + u(y^s) \cdot \frac{dp}{dB^*} + [1 - p(B^*)] \cdot \frac{du}{dy^h} \cdot (-w) + u(y^h) \cdot \left(-\frac{dp}{dB^*}\right) = 0$$

Equation 3-2

This simplifies to:

$$\frac{dp}{dB^*} \cdot [u(y^s) - u(y^h)] = w \cdot \{p(B^*) \cdot \frac{du}{dy^s} + [1 - p(B^*)] \cdot \frac{du}{dy^h}\}$$

Equation 3-3

The term in {brackets} on the right-hand side represents the expected value of marginal utility and can be denoted as $\frac{dEU}{dy}$ and so the first-order condition further simplifies to:

$$\frac{dp}{dB^*} \cdot [u(y^s) - u(y^h)] = w \cdot \frac{dEU}{dy}$$

Equation 3-4

The first term in Equation 3-4 ($\frac{dp}{dB^*}$) represents the change in probability of getting sick from a marginal increase in healthy behaviors. This will be a negative quantity since I assumed above that $\frac{dp}{dB} < 0$. The second term on the left-hand side $u(y^s) - u(y^h)$ represents the difference in utility between the two states of the world and is also negative because of the sign restrictions placed on the parameters. So the left-hand side amounts to the decreased probability

[32] For simplicity, I denote income in each state as y^s and y^h in equations 1-4. Note, however, that *y* is always a function of *L*, *w*, and *B*.

of suffering the utility difference and thus represents the *marginal benefit* from an incremental increase in healthy behaviors.

The right-hand side amounts to the agent's shadow price of time multiplied by the marginal utility of income. Income Y is decreasing in B and so the derivative term represents the decrease of utility associated with additional healthy behaviors in both states of the world. We can thus interpret the right-hand side of the equation as the *marginal cost* of healthy behaviors. Therefore, the first order condition specified in Equation 3-4 states that utility-maximizing agents will choose an optimal level of B^* such that the marginal gain from healthy behaviors is equal to the marginal cost of healthy behaviors. B^* is increasing in out-of-pocket cost L.

3.2.1 Impact of the Policy

Next, I incorporate the Part D policy into this framework. Part D affects y^s by reducing the cost of being sick (L) and thus increasing the value of y^s. The policy reduces L by making prescription drug insurance available to agents, which in turn reduces the agent's out-of-pocket cost of medical care. The rise in y^s narrows the utility gap between the healthy state and sick state, which in turn reduces the marginal gain from healthy behaviors (i.e. the left-hand side of the equation). Since the left-hand side of the first-order condition is now less than the right-hand side of the equation, agents must respond by lowering their optimal choice of B^*. (I assumed above that $\frac{dp}{dB} < 0$ and $\frac{d^2p}{dB^2} > 0$.) Thus, the model predicts that a positive shock in healthcare access, such as the one associated with Part D, would undermine the agent's incentive to engage in the healthy behavior. Specifically, Part D lowers the price of prescription drugs for people who are Medicare eligible, so we would expect the Medicare eligible to use less of the healthy

diet substitute, whereas the policy does not change the price of drugs for younger adults who are ineligible for Medicare, so we should expect no change in diet for them.

3.3 Data and Measures

The National Health and Nutrition Examination Survey (NHANES) is a biennial survey conducted by the Centers for Disease Control and Prevention (CDC). The NHANES provides detailed dietary intake data that respondents consume during the 24-hour period prior to the interview. It is well-suited for my study because it contains detailed information on daily total nutrient intakes from foods and beverages for survey participants. The main outcomes I examine in this analysis are daily consumption of calories (kcal), carbohydrates (gm), total fat (gm), protein (gm), total saturated fatty acids (gm), cholesterol (mg), and sugar (gm). In the Appendix, I present estimates for additional outcomes, including consumption of calcium and other nutrients. The sample is nationally representative and consists of about 5,000 individuals each year. For my main analysis, I restrict the sample to the 2000-12 NHANES. Table 3-1 displays descriptive statistics of the NHANES study sample.

I also present supplementary results using the 2001-09 Behavioral Risk Factors Surveillance System (BRFSS). The BRFSS contains individual-level data on eating habits and other health behaviors for 500,000 individuals per year. This dataset is widely used to study the effects of state and federal policies in individuals' health behaviors (Barbaresco et al., 2015; K. Simon et al., 2017). It includes a number of questions related to consumption of fruits and vegetables. The main outcomes I examine in this analysis are: number of servings per month of fruit juice, fruit, green salad, potatoes, carrots, and other vegetables. For my primary analysis, I restrict the analytical sample to those aged 60 to 70; I also omit those aged 65 from the sample. Appendix Table 3- 2 provides demographic characteristics of the BRFSS sample.

3.4 Empirical Methods

This section describes a model intended to estimate the causal impact of prescription drug coverage on health behaviors. Without employing causal inference methods, an ordinary least squares model regressing health behavior on prescription drug coverage may be biased if unobservable factors that influence an individual's drug coverage are correlated with unobservable factors that determine the individual's health behaviors. Without controlling for these unobservable factors, drug coverage would positively correlate with the error, and the estimated ex-ante moral hazard effect would be overstated. On the other hand, those who had insurance before 2006 could have been healthier (if insurance companies screened out high-risk individuals), in which case the moral hazard effect would be understated (healthier people are less likely to engage in ex-ante moral hazard). Therefore, conventional least squares regression will yield biased results.

In order to obtain causal estimates, I exploit plausibly exogenous variation in prescription drug insurance enrollment generated by the introduction of Medicare Part D. I use a difference-in-differences (DD) study design that compares outcomes among people aged 66-70 (the treatment group) to those aged 60-64 (the control group), before and after the 2006 policy change. My baseline model is:

$$Y_{igst} = \alpha + \beta(Treatment_g * Post_t) + \gamma \mathbf{X}_{igst} + \phi Unemp_{st} + \eta Age_i + \delta State_s + \vartheta Time_t + \varepsilon$$

Equation 3-5

where Y_{igst} represents a diet-related outcome for individual i in age group g living in state s at time t, $Treatment_g$ is an indicator equal to one if the individual is 66-70 and zero if the individual is 60 to 64, $Post_t$ is an indicator equal to one if the time period is after January 2006, $\mathbf{X}_{igst}$ is a vector of demographic control variables, $Unemp_{st}$ is the state unemployment rate, Age_i is a vector of age-fixed effects, $State_s$ is a vector of state-fixed effects, and $Time_t$ is a vector of time-

fixed effects. Standard errors are heteroskedasticity-robust, and regressions include BRFSS survey weights.

The coefficient β represents the causal effect of prescription drug coverage on Y provided that the control group is good counterfactual for the treatment group, i.e. in the absence of Medicare Part D, both groups would have trended similarly. In order to assess the plausibility of this assumption, I conduct parallel trends tests using an event study approach. I estimate a model similar to Equation 3-5, but I replace the $Treatment_g$*$Post_t$ interaction term with a vector of interactions for each year indicator and $Treatment_g$, excluding the year 2005 as the base year. I conduct an F-test to test whether all the pre-2006 interaction terms are jointly equal to 0. A p-value greater than 0.10 means that I cannot reject the null hypothesis of equal trends between the treatment and control groups before 2006. This provides confidence that the control group is a good counterfactual for the treatment group.

3.4.1 Additional Empirical Tests

I also assess whether impacts were stronger among demographic and socioeconomic groups most impacted by Part D by estimating Equation 3-5 separately for subsamples stratified by income, race/ethnicity, educational attainment, and geographic region. Before Part D, drug coverage was concentrated among those with income over 200% FPL and those below the poverty line (Long, 1994). Another study found that those who gained most from Part D tend to be low educated, low income, black, and more likely to come from some regions (e.g., east and west South Central) (Kaestner & Khan, 2012). Assuming that it was indeed changes in prescription drug insurance that principally caused changes in health behaviors, we should expect changes between 2004 and 2007 to be greater for those with incomes between 100 and 200% FPL and those with low education. By honing in on those who were more likely to have

gained drug coverage through Part D, I assess whether my estimates are stronger for these groups.

3.5 Results

I first assess visual trends in diet outcomes over time. Figure 3-3 and Figure 3-3 show annual means for each of the NHANES and BRFSS outcome variables separately for the treatment group and the control group. For nearly all outcomes, the elderly treatment group and non-elderly control groups appear to trends similarly over the pre-2006 period. There is visual evidence of a relative decline in consumption of certain fruits and vegetables after 2006.

Table 3-2 presents DD results from the NHANES analysis. I find that Part D increased total calorie consumption by 76.9 calories per day (a 4.4 percent increase over pre-2006 levels), increased carbohydrate consumption by 12.6 grams per day (5.8 percent increase), increased total fat consumption by 2.9 grams per day (4.6 percent increase), increased total saturated fatty acid consumption by 1.5 grams per day (7.1 percent increase), and increased sugar consumption by 12.0 grams per day (12.1 percent increase). Most of these increases were concentrated among unmarried males. I examine additional outcomes, and results are displayed in Appendix Table 3-3. I find increases in total monounsaturated fatty acids, reductions in dietary fiber intake, reductions in Vitamin B6, reductions in Vitamin K, reductions in Beta-carotene, and increases in caffeine consumption. Again, the majority of these changes came from unmarried men.

Appendix Figure 3- 1 displays results from the event study tests for parallel trends for the NHANES outcomes. For most outcomes, there is no evidence of differential trends between the treatment and control groups in the pre-2006 years. This increases confidence that the DD assumptions are met and that the DD estimates can be interpreted as causal.

Table 3-3 displays the DD results from the BRFSS analysis. I find that Part D significantly reduced consumption of fruit by 1.2 servings per month (a 4.2 percent decline compared to pre-2006 levels), reduced green salad consumption by 0.6 services per month (3.5 percent decline), and reduced the consumption of other vegetables by 1.2 servings per month (2.9 percent decrease). There was no significant change in consumption of fruit juice, potatoes, and carrots. Appendix Table 3- 4 presents results from the pre-trends analysis. I find no evidence that the treatment and control groups followed differential trends before 2006 for any of the six outcomes, which increases confidence in the causal interpretation of these results.

3.6 Discussion

This study provides some of the first evidence that expanded pharmaceutical access unintentionally worsens individuals' diet habits. I exploit the introduction of Medicare Part D, and find significant increases in total saturated fat and sugar consumption and reductions in fruit and vegetable consumption; changes were concentrated among unmarried men. These findings are informative about the presence of ex-ante moral hazard as well as substitutability between prescription drugs and diet. These results also support previous studies which find no significant impact of Part D on cholesterol levels, in spite of increased use of statins and other antilipidemic drugs (Hanlon et al., 2013).

Part D has numerous large benefits for health and financial outcomes for the elderly, and thus policymakers should not interpret these findings as rationale for cutting back on prescription drug coverage for older people. However, it is important that they are aware of potential negative spillover effects and supplement coverage accordingly. For example, Medicare does not currently cover services of nutritionists or dieticians. There is some empirical evidence that informational nudging can help people make healthier food choices (Nikolova & Inman, 2015;

Oster, 2018). Policymakers may consider expanding Medicare's coverage of nutrition services as a way to combat the unintended negative side-effects of Part D on health behaviors.

This analysis is subject to several limitations. The control group might be contaminated because people close to age 65 know that they will get Part D in the future, and therefore there may be anticipatory effects. The Medicare Modernization Program also created the Medicare Discount Drug Card Program which took effect in early 2004 and helped Medicare recipients receive discounts on their Rx drugs during the two years prior to Part D implementation. I conduct robustness checks omitting the years 2003-04 and individuals aged 63-64, and find that the results persist even after accounting for potential anticipatory effects of the policy.

Figures

Figure 3-1. Example Functional Form of p(B)

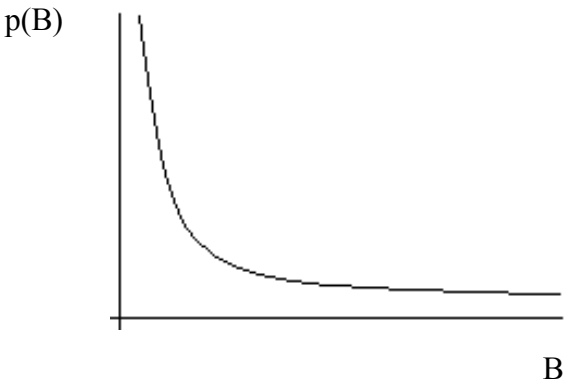
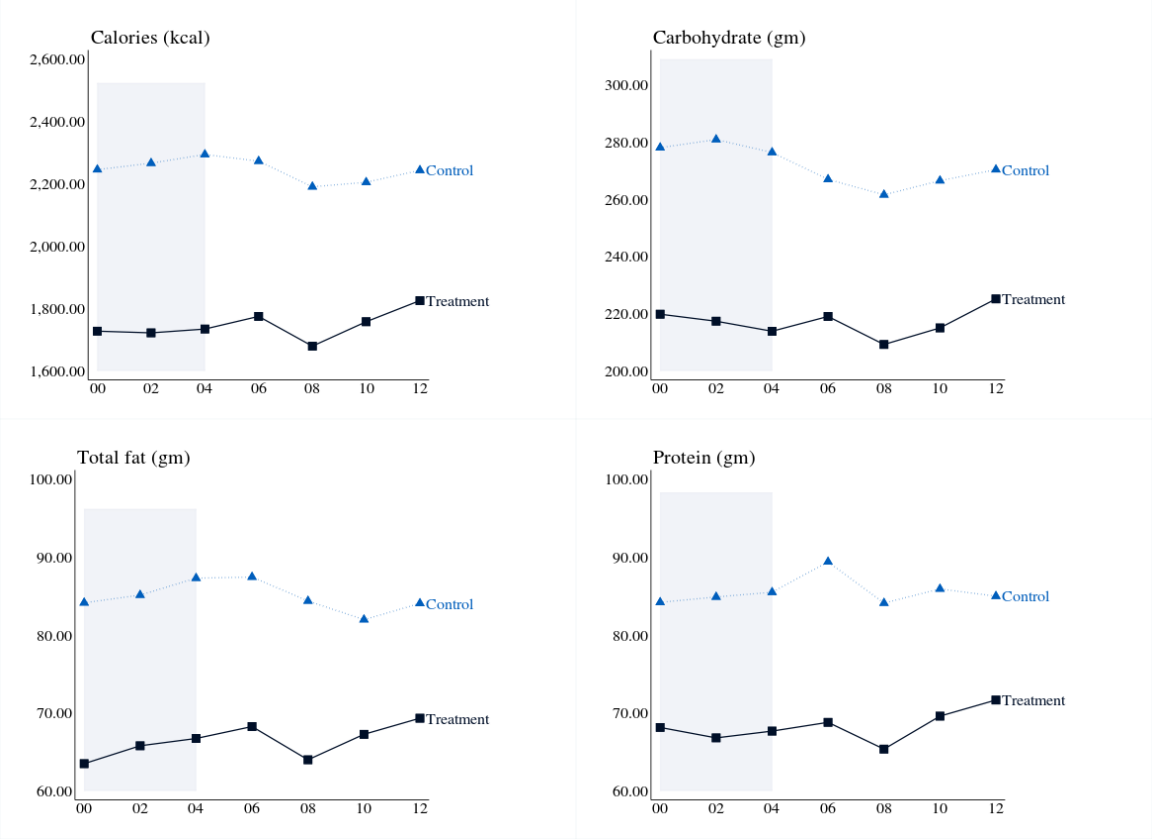
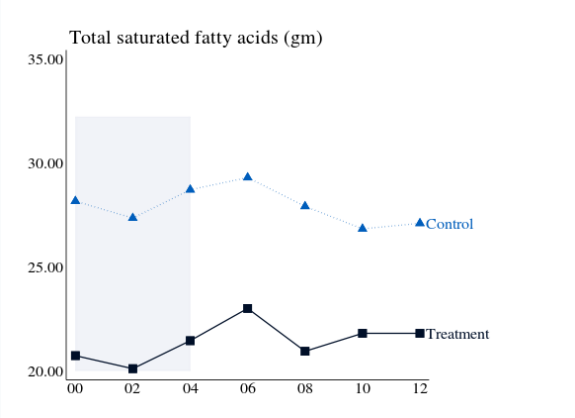
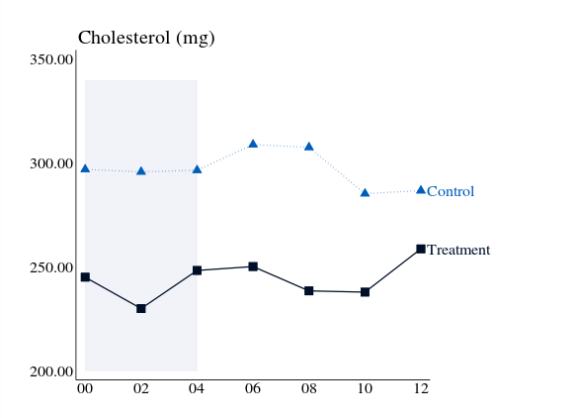
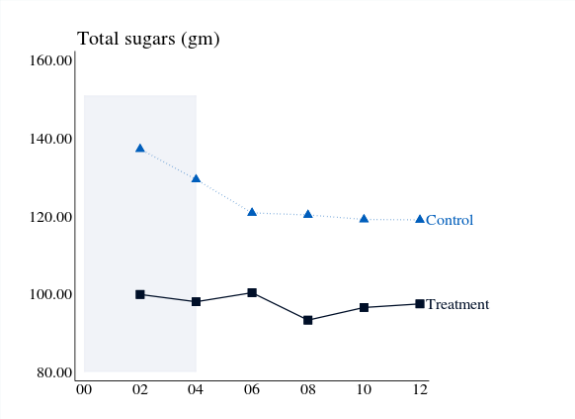


Figure 3-2. Trends in NHANES Diet Outcomes



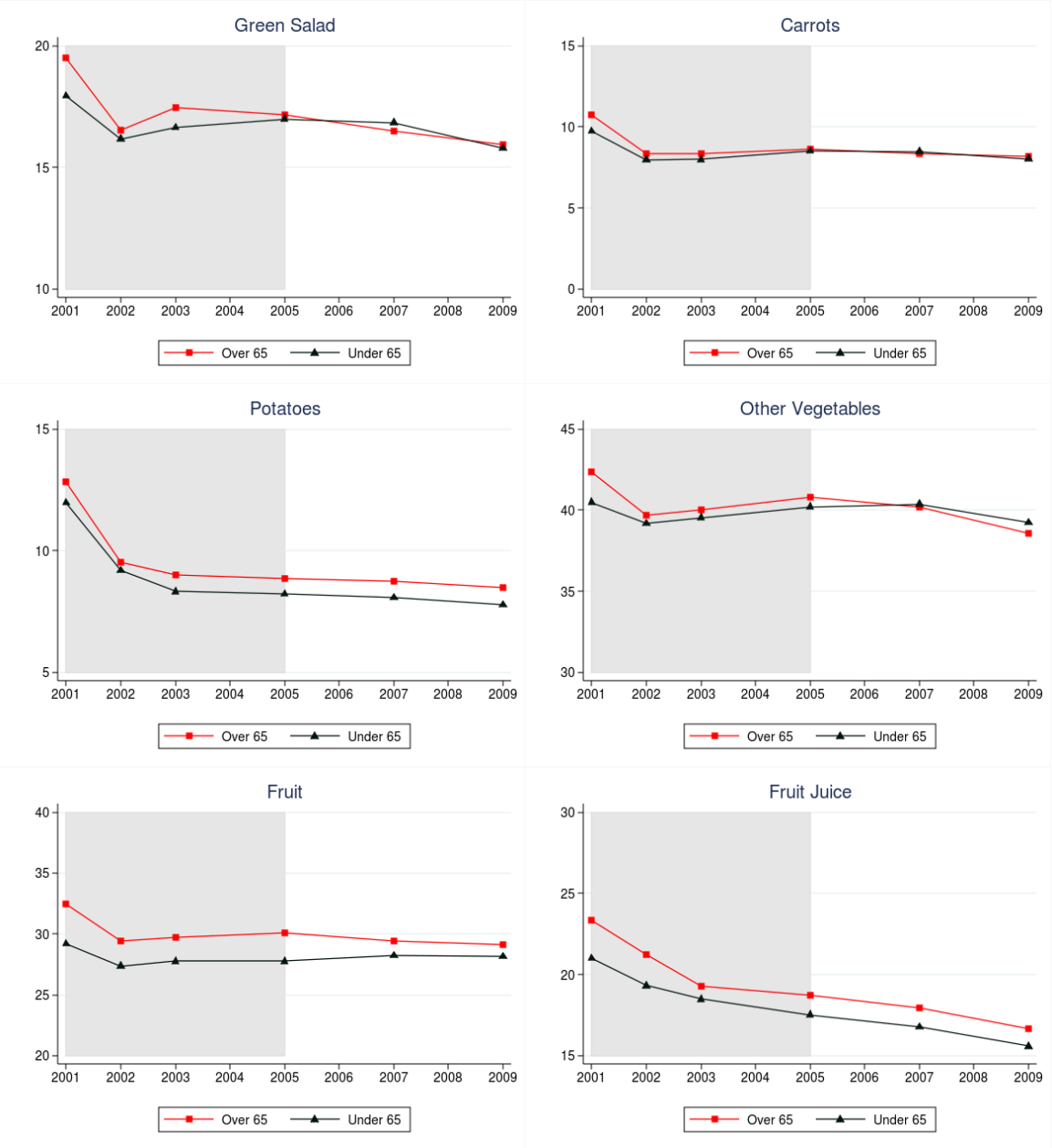






Source: Author's calculations based on NHANES 2000-2012. The treatment group is adults aged 65 and older; the control group is adults aged 25 to 64. Data is adjusted by NHANES sample weights.

Figure 3-3. Trends in BRFSS Diet Outcomes



Source: Author's calculations based on BRFSS 2001-2009. Sample is restricted to include adults aged 60 to 70; adults aged 65 are excluded from the sample. The treatment group is adults aged 60 to 64; the control group is adults aged 66 to 70.

Tables

Table 3-1. Demographic Characteristics of the NHANES Sample

	Control Group	Treatment Group
Age	43.65	73.46
Male	0.487	0.442
Educational attainment		
Less than high school	0.164	0.278
High school	0.232	0.276
Some college	0.305	0.240
College or more	0.299	0.206
Race/ethnicity		
White, non-Hispanic	0.701	0.823
Black, non-Hispanic	0.113	0.0784
Other race, non-Hispanic	0.0572	0.0347
Hispanic	0.129	0.0636
Household income (thousands)	65.22	46.55
Married	0.610	0.587
Household size	3.101	2.026
Observations	20,679	7,725

Source: Author's calculations based on NHANES 2000-2012. The treatment group is adults aged 65 and older; the control group is adults aged 25 to 64. Data is adjusted by NHANES sample weights.

Table 3-2. DD Results for Impact of Medicare Part D on NHANES Diet Outcomes

	Pooled	Unmarried Females	Married Females	Unmarried Males	Married Males
Calories (kcal)	76.936***	61.601	59.452	310.927***	29.826
	(27.023)	(47.862)	(45.373)	(78.981)	(51.068)
	[μ=1,727.8]	[μ=1,506.9]	[μ=1,563.8]	[μ=1,824.3]	[μ=2,023.4]
Carbohydrates (gm)	12.596***	11.211*	7.610	48.993***	4.341
	(3.671)	(6.441)	(6.396)	(10.587)	(6.960)
	[μ=216.8]	[μ=192.3]	[μ=200.9]	[μ=216.3]	[μ=249.6]
Protein (gm)	0.182	-2.072	1.438	7.836**	-1.044
	(1.190)	(2.062)	(2.050)	(3.557)	(2.261)
	[μ=67.5]	[μ=59.8]	[μ=59.1]	[μ=70.6]	[μ=79.9]
Total fat (gm)	2.888**	2.728	3.038	10.379***	1.289
	(1.352)	(2.421)	(2.392)	(3.663)	(2.577)
	[μ=65.4]	[μ=57.5]	[μ=59.2]	[μ=68.2]	[μ=77.2]
Cholesterol (mg)	2.796	-13.879	6.495	4.038	13.627
	(8.046)	(13.150)	(16.594)	(23.034)	(14.903)
	[μ=241.4]	[μ=211.7]	[μ=213.6]	[μ=289.4]	[μ=277.1]
Total saturated fatty acids (gm)	1.477***	1.321	2.035**	3.177**	0.693
	(0.482)	(0.865)	(0.834)	(1.385)	(0.919)
	[μ=20.8]	[μ=18.1]	[μ=18.3]	[μ=22.9]	[μ=24.6]
Sugar (gm)	11.958***	6.962	7.792	35.385***	7.561
	(2.635)	(4.470)	(4.779)	(7.193)	(5.113)
	[μ=98.8]	[μ=90.0]	[μ=95.8]	[μ=97.6]	[μ=110.1]
Observations	28,404	7,009	7,554	5,024	8,817

Source: Author's calculations based on NHANES 2000-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the treatment group indicator, sex, age, race/ethnicity, educational attainment, marital status, household income, household size, and year-fixed effects. Data is adjusted by NHANES sample weights. Robust standard errors are in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3-3. DD Results for Impact of Medicare Part D on BRFSS Diet Outcomes

	Pooled	Unmarried Females	Married Females	Unmarried Males	Married Males
Fruit Juice	-0.284 (0.403) [μ=17.52]	-0.750 (0.721)	0.251 (0.540)	-0.558 (1.424)	-0.210 (0.756)
Fruit	-1.186*** (0.440) [μ=27.76]	-1.305 (0.921)	0.080 (0.814)	-2.388* (1.223)	-1.631** (0.692)
Green Salad	-0.590** (0.268) [μ=16.99]	-0.007 (0.564)	-0.581 (0.503)	-1.689** (0.770)	-0.521 (0.421)
Potatoes	0.006 (0.157) [μ=8.21]	-0.171 (0.281)	-0.465* (0.259)	0.185 (0.555)	0.360 (0.272)
Carrots	-0.338 (0.223) [μ=8.53]	-0.019 (0.422)	-0.570 (0.393)	-0.285 (0.476)	-0.318 (0.389)
Other Vegetables	-1.158*** (0.446) [μ=40.21]	-0.103 (0.856)	-2.271*** (0.817)	1.810 (1.456)	-1.660** (0.714)
Observations	227,526	66,383	67,400	28,061	65,682

Source: Author's calculations based on BRFSS 2001-2009. Sample is restricted to include adults aged 60 to 70; adults aged 65 are excluded from sample. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for sex, age, race/ethnicity, education, marital status, unemployment status, household income, household size, state unemployment rate, state-fixed effects, and quarter/year-fixed effects. Data is adjusted by BRFSS sample weights. State-clustered standard errors are in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4 Consumption Effects of Health Insurance Expansions for Young Adults: Evidence from Scanner Data

Abstract

Improving financial outcomes is a stated policy goal of health insurance expansions, but little is known about how health insurance impacts consumption patterns, particularly among young adults. I exploit a large increase in insurance coverage for young adults generated by the Affordable Care Act's 2010 dependent coverage provision. Before this policy, young adults faced high uninsurance rates and large out-of-pocket medical expenses. I use detailed data on daily purchases by a panel of 60,000 households and apply quasi-experimental study designs to assess the dependent coverage provision's impact on consumption among young adults. I measure both income effects of health insurance, by studying the expansions' impacts on total consumer purchases, and substitution effects, by studying impacts on purchases of specific products. I find that expanded insurance eligibility increases total consumer spending by 8 percent; this was driven by increases in purchases of food, alcohol, contraceptives, and over-the-counter medications. This study provides important evidence of the role that health insurance expansions can play in improving consumption and financial outcomes among vulnerable populations.

4.1 Introduction

Health insurance provides people with protection against potentially catastrophic medical expenses that may result from illness or injury. Health care costs are high in the United States, particularly for acute and emergent care. The uninsured therefore face considerable financial risk. Before the implementation of the Affordable Care Act's (ACA's) dependent coverage provision in 2010, uninsured young adults were particularly vulnerable. Uninsured young adults spent on average more than 50 percent of their total annual medical expenses, or $575 per year, out-of-pocket (OOP), whereas insured young adults spent only 15 percent of their annual medical expenses, or $384 per year, OOP.[33] It is plausible that insuring young adults would reduce this financial risk, free up their income and precautionary savings, and improve their financial outcomes.

Improving financial outcomes is often a stated policy goal of insurance expansions. However, the causal effect of insurance expansions on financial outcomes can be challenging to measure because of the endogeneity between income and insurance eligibility. Income and assets are often determinants of eligibility for public health insurance, and so people may have incentives to distort or misreport income in order to maintain insurance eligibility. There may also be unobserved factors, such as risk aversion, that are correlated with both financial outcomes and insurance enrollment. Moreover, there could be reverse causality if financial distress itself causes poor health. To circumvent these issues, I use consumers' purchasing power as a proxy measure for financial wellbeing. I measure how people's monthly household purchases change in response to increased health insurance eligibility. My identification strategy

[33] Author's calculations based on sample of 19-26 year olds from the 2006-08 Medical Expenditure Panel Survey.

exploits a recent health insurance expansion resulting from the ACA dependent coverage provision in 2010.

4.1.1 The Dependent Coverage Provision

The ACA significantly expanded health insurance coverage in the United States (Frean, Gruber, & Sommers, 2017) through various provisions. The one that I focus on is the 2010 dependent coverage provision of the ACA, also known as the Young Adult Mandate. The Mandate was one of the first coverage provisions of the ACA; it required insurers to allow children to remain on their parents' private insurance plans until age 26. Before the implementation of the provision in September 2010, in most states, children were no longer able to remain on their parents' plans upon turning 19. Even in those states that had some sort of provision for continuing parental insurance coverage for young adults, coverage was typically dependent on being a student, being unmarried, or being financially dependent on parents.

Consequently, young adults faced some of the highest uninsurance rates of all age groups in the country. In 2009, about 31 percent of adults aged 19-25 (or 9.2 million individuals) lacked insurance coverage (Busch, Golberstein, & Meara, 2014). Previous studies show that the dependent coverage provision led to substantial increases in insurance, reducing uninsurance rates among targeted individuals by about 10 percentage points (or 3 million young adults) by the end of 2011 (Akosa Antwi, Moriya, & Simon, 2013; Sommers, Buchmueller, Decker, Carey, & Kronick, 2013). Moreover, previous studies have found that the dependent coverage provision reduced large and uncertain out-of-pocket medical expenses for young adults (Ali, Chen, Mutter, Novak, & Mortensen, 2016; Busch et al., 2014). If young adults reallocated this income to consumption, we should expect to see increases in consumer purchases.

4.1.2 Literature on Health Insurance and Financial Wellbeing

Previous studies have examined the relationship between health insurance and financial outcomes in different settings. Appendix 4-A provides a comprehensive review of this literature. Others have studied the impact of the ACA dependent coverage provision on financial outcomes other than consumption (Blascak & Mikhed, 2018; Han, 2016). Blascak & Mikhed (2018) use data on consumer credit scores and find that the provision decreased the number of delinquencies and the probability of filing for bankruptcy among young adults; effects were strongest for individuals living in counties with the highest pre-2010 uninsurance rates. Han (2016) studies student loan debt and finds that the provisions reduced the student loan default rate and increased repayment rates of student loans.

There is a large literature which examines the effects of health insurance on financial outcomes for other populations. For example, studies find that Medicare decreased OOP medical expenditure risk (Finkelstein & McKnight, 2008), reduced contact with collection agencies (Barcellos & Jacobson, 2015), and increased investments in risky assets (Angrisani, Atella, & Brunetti, 2018; Christelis, Georgarakos, & Sanz-de-Galdeano, 2017) among elderly people. The Medicare Part D provision, in particular, reduced OOP medical expenditures (Engelhardt & Gruber, 2011) and increased investments in risky assets (Ayyagari & He, 2016) by expanding prescription drug coverage for the over-65 population.

There are also a number of studies evaluating the financial impacts of Medicaid expansion for low-income people. These papers report strong and consistent increases in credit scores and retirement savings, and reductions in OOP medical expenditures, medical and non-medical debt, payday loans, delinquencies, personal bankruptcy, and evictions (Allen, Swanson, Wang, & Gross, 2017; Brevoort, Grodzicki, & Hackmann, 2017; Caswell & Waidmann, 2017;

Dillender, 2017; E. Gallagher, Gopalan, Grinstein-Weiss, & Sabat, 2018; Gross & Notowidigdo, 2011; Hu, Kaestner, Mazumder, Miller, & Wong, 2018; Kino, Sato, & Kawachi, 2018; Leininger, Levy, & Schanzenbach, 2010; Miller, Hu, Kaestner, Mazumder, & Wong, 2018; Sommers & Oellerich, 2013). Another study exploits income eligibility thresholds to receive ACA marketplace subsidies and finds that subsidized coverage reduces the probability of being delinquent on home payments (E. A. Gallagher, Gopalan, & Grinstein-Weiss, 2017).

Others have evaluates causal effects of health insurance on financial outcomes in the context of the Oregon health insurance experiment (Finkelstein et al., 2012), the Massachusetts health care reform (Mazumder & Miller, 2016), and the Tennessee Medicaid disenrollment (Argys, Friedson, Pitts, & Tello-Trillo, 2017). Nearly all these studies find strong evidence that health insurance increases credit scores, and reduces debt, delinquencies, and other measures of financial strain.

While there is a large body of evidence showing that health insurance improves financial outcomes such as credit scores, debt, and bankruptcy, we know very little about the effects of expansion on consumption itself. There are a few exceptions. For example, Dillender (2017) applies an instrumental variables strategy to estimate the effect of an additional family member becoming eligible for Medicaid and finds that while insurance reduces medical spending, there is no detectable effect on non-health spending. Levy et al. (2019) exploits the ACA Medicaid expansion and draws similar conclusions, that Medicaid reduces health spending but does not change non-health consumption. Leininger et al. (2010), on the other hand, finds increases in overall expenditures (mostly transportation and retirement savings) resulting from expansions of the Children's Health Insurance Program. All three of these studies use the Consumer Expenditure Survey (CEX) and focus on low-income populations.

To my knowledge, this paper is the first to study the effect of health insurance on consumption for young adults below age 26. Adults aged 19-26 are usually in the early stages of their career or full-time students and therefore financially vulnerable. Financial outcomes during this stage of life can have important implications for later-life outcomes. I exploit the implementation of the ACA dependent coverage provision and use detailed data on household purchases from the Nielsen Household Consumer Panel (NHCP), a dataset that has powerful applications in public policy but is not yet widely used in the field. I find that expanded insurance eligibility increases total consumer spending, particularly in the categories of food, contraceptives, and over-the-counter medications.

4.2 Data

I use the Nielsen Household Consumer Panel (NHCP), a panel survey that reports all the household products purchased daily by a panel of 60,000 US households. Variables include household demographics (including household income, race/ethnicity, household size, and age and educational attainment of all members in the household), geographic identifiers (to the zip code level), and product characteristics (to the UPC code level). After each shopping trip, households report their purchases using handheld barcode scanners and keypads provided by Nielsen. Participating households are asked to scan all their purchases, including groceries, drugs, small appliances, and mass merchandise products. About 80 percent of households are retained from year to year, and there are around 60,000 active households (that meet Nielsen's reporting requirements) in any given year.

The Nielsen has several advantages over the Consumer Expenditure Survey (CEX), the main dataset used in previous studies of health insurance and consumption (Dillender, 2017; Leininger et al., 2010; Levy, Buchmueller, & Nikpay, 2019). First, the use of scanners and

receipts in real time increases the accuracy of reported expenditures. Second, there is a large amount of detail available on products; approximately 3.4 million UPC codes have been purchased at some point by households in the NHCP. Thus, I am able to differentiate between branded and generic products. Third, the NHCP provides detailed geographic identifiers down to the zip code level, which allows me to estimate triple differences models exploiting variation in pre-2010 uninsurance rates across counties. However, unlike the Consumer Expenditure Survey, the Nielsen data does not include information on expenditures for goods and services used outside the home, including purchases at bars and restaurants.

4.3 Methods

To assess the impact of the ACA dependent coverage provision on consumer purchases, I exploit the fact that only those below the age of 26 were eligible for the provision. I compare consumption outcomes for households impacted by the policy with those unaffected because they were too old, before and after the implementation of the policy in 2010. The treatment group is defined as households in which all members are aged 25 and younger, and the control group consists of households in which all members are aged 27 to 31; households with members of other ages are omitted from analysis. Households in which at least one member is below 25 *and* at least one other member is 27 to 31 are also omitted from analysis. A similar difference-in-differences (DD) empirical approach has been used in the past literature to evaluate the ACA dependent coverage provision (Akosa Antwi, Ma, Simon, & Carroll, 2016; Akosa Antwi et al., 2013; Barbaresco et al., 2015; Depew, 2015). My specific DD model is:

$$Y_{igst} = \alpha + \beta\,(Treatment_g \; X \; Post_t) + \delta Treatment_g + \gamma \boldsymbol{X}_{igst} + \eta Household_i + \phi Time_t + \varepsilon \quad (1)$$

where Y_{igst} represents an insurance outcome for individual i in age group g living in state s in year t. *Treatment* is a binary variable equal to 1 if the household is in the treatment group

defined above and equal to 0 if the household is not in the control group. *Post* is a binary variable equal to 1 if the time period is after the policy implementation (i.e. 2010 to 2013) and equals 0 if the time period is before the policy implementation (i.e. 2004 to 2009). $\boldsymbol{X}$ is a vector of demographic control variables, including age of householder, educational attainment of householder, race/ethnicity, and marital status. *Household* is a vector of household state fixed effects, and *Time* is a vector of month-year fixed effects. I estimate linear probability models because they typically give reliable estimates of average effects (Angrist & Pischke, 2009). Standard errors are clustered at the household level to allow for common unobserved shocks to occur to the same household over time.

I estimate Equation (1) for households' monthly spending on food and all household products reported in Nielsen, adjusted for inflation. I also classify purchases based on department and estimate models for inflation-adjusted monthly spending on 1) health and beauty, 2) alcohol, 3) non-food grocery, 4) general merchandise, 5) all food, 6) frozen food, 7) dairy, 8) deli, 9) packaged meat, and 10) fresh produce.

A key assumption of the DD model is that in the absence of treatment, outcomes among the treatment group would have followed the same trend as those among the control group. If this assumption holds, then the DD coefficient β in equation (1) identifies the causal effect of the policy change on the outcome. I assess the validity of this assumption by comparing pre-2010 trends in outcomes in the treatment and control groups. I do this by first visually assessing graphs of the trends and then estimating regressions that interact the *Treatment* indicator with year indicator variables for all years except 2009, which is the base year. If the treatment and control groups indeed followed similar trends before 2010, then the coefficient on the pre-2010

interaction terms should be jointly equal to 0. I test the null hypothesis that all pre-2010 interaction terms equal 0 using a joint F-test.

4.4 Results

Table 4-1 presents demographic characteristics of the treatment and control groups. There are 11,277 households in the treatment group and 56,138 households in the control group. The treatment group tends to be less wealthy and more likely to have a head of household who is not working and who is not college educated.

Table 4-2 displays the pre-2010 means and DD coefficient estimates for all outcomes described in the Methods section above. The dependent coverage provision led to a $26.79 increase in the total monthly purchases of households with eligible young adults ($p<0.10$). Before 2010, these households spent an average of $331 per month on food and household purchases, so this represents an 8.1 percent increase over pre-2010 levels. This change was largely driven by increases in monthly purchases of alcohol ($2.66 increase in monthly purchases, $p<0.05$), frozen foods ($2.51 increase, $p<0.10$), and dairy ($1.54 increase, $p<0.10$). There was no significant change in purchases in other departments, including health & beauty, non-food grocery, general merchandise, all food, deli, and packaged meat. In subsequent analyses (available on request), I study finer categories of products and find increases in purchases of over-the-counter medications and contraceptives.

In Table 4-3, I expose my model to a number of sensitivity analyses. Column (1) replicates the results of the baseline model in Table 2 for comparison purposes. In Column (2), I include a vector of state fixed effects on the right hand side. In Column (3), I measure the time fixed effects as two separate vectors of year fixed effects and calendar month fixed effects. In Column (4), I include a vector of age fixed effects for each householder on the right hand side.

For the most part, these adjustments do not substantially change the magnitude or direction of the coefficients from the baseline model.

Table 4-4 displays results from the event study models. Each column is a separate regression, and the column header lists the outcome variable of the regression. I find that for total spending, none of the pre-2010 interaction terms are significant, suggesting that the treatment and control groups trended similarly before the enactment of the dependent coverage provision. This gives confidence that the control group serves as a good counterfactual to the treatment group. For most of the remaining outcomes also, I am unable to reject the null hypothesis that all pre-2010 interaction terms equal 0. The only exceptions are general merchandise and frozen foods, but for both these outcomes the pre-2010 interaction terms are negative, suggesting that the bias is in the opposite direction.

4.5 Discussion

The ACA dependent coverage provision increased total spending among young adults, particularly in the categories of food (dairy and frozen foods), alcohol, contraceptives, and over-the-counter medications. These results provide evidence that expanding health insurance for young adults increases their consumption power. This increased purchasing power is of particular importance to young adults, who often face high debt burdens from education-related debt and low wages due to limited work experience. Thus, expanding insurance coverage to this vulnerable group may be a way to improve their financial outcomes, which was one of the stated goals of the Affordable Care Act.

Tables

Table 4-1. Demographic Characteristics of Treatment and Control Groups

	Control Group (All members aged 27-31)	Treatment Group (All members below age 26)
Income (thousands)	58.43	36.36
Percent of Federal Poverty Line	493.1	284.0
Household size	1.478	1.942
Married	0.375	0.366
Have children?	0	0.241
Female householder?	0.819	0.858
Female householder age	28.67	23.60
Female householder not working	0.124	0.306
Female householder at least high school educated	0.993	0.965
Female householder at least college educated	0.751	0.445
Male householder?	0.617	0.635
Male householder age	29.04	23.75
Male householder not working	0.0682	0.123
Male householder at least high school educated	0.987	0.923
Male householder at least college educated	0.645	0.363
White	0.788	0.802

Black	0.0908	0.0704
Asian	0.0539	0.0525
Other race	0.0678	0.0748
Hispanic	0.0697	0.105
Observations	56,138	11,277

Notes: Author's calculations based on Nielsen 2004-2013. Treatment group is defined as householders in which all members are below age 26. Control group is defined as households in which all members are aged 27 to 31.

Table 4-2. DD Estimates for Impact of ACA Dependent Coverage Provision on Households' Monthly Spending

	Pre-2010 Mean	DD Estimate	Percent Change
Total Spending	331.4	26.790*	8.1%
		(15.411)	
Health & Beauty	19.96	1.733	-
		(1.514)	
Alcohol	5.341	2.657**	49.7%
		(1.164)	
Non-Food Grocery	26.63	0.990	-
		(1.270)	
General Merchandise	14.50	1.985	-
		(2.982)	
All Food	264.9	19.425	-
		(12.855)	
Frozen Foods	21.06	2.505*	11.9%
		(1.516)	
Dairy	19.13	1.535*	8.0%
		(0.802)	
Deli	4.684	1.092	-
		(1.111)	
Packaged Meat	6.408	0.088	-
		(0.412)	
Observations		67,196	

Notes: Author's calculations based on Nielsen 2004-2013. Treatment group is defined as householders in which all members are below age 26. Control group is defined as households in which all members are aged 27 to 31. All outcomes are measured in dollars spent per month, adjusted for inflation. Pre-2010 mean column displays pre-2010 mean for the treatment group. DD estimate column displays coefficient on interaction of Post-2010 and treatment indicators. All regressions also control for treatment indicator, race/ethnicity, marital status, age and educational attainment of each householder, month-year fixed effects, and household fixed effects. Standard errors are clustered at the household level.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 4-3. Sensitivity Analyses

	Baseline (1)	State FE (2)	Separate Year and Month FE (3)	Age FE (4)
Total Spending	26.790*	22.577	25.314*	25.177*
	(15.411)	(14.780)	(15.327)	(14.088)
Health & Beauty	1.733	1.689	1.647	2.320
	(1.514)	(1.569)	(1.522)	(1.498)
Alcohol	2.657**	2.549**	2.758**	2.011*
	(1.164)	(1.163)	(1.163)	(1.158)
Non-Food Grocery	0.990	1.008	0.765	1.801
	(1.270)	(1.241)	(1.264)	(1.251)
General Merchandise	1.985	0.556	1.769	1.100
	(2.982)	(2.022)	(2.956)	(2.301)
All Food	19.425	16.775	18.375	17.946
	(12.855)	(12.710)	(12.787)	(11.848)
Frozen Foods	2.505*	2.389	2.439	2.986**
	(1.516)	(1.528)	(1.515)	(1.497)
Dairy	1.535*	1.549*	1.517*	1.623**
	(0.802)	(0.811)	(0.799)	(0.803)
Deli	1.092	1.045	1.204	0.692
	(1.111)	(1.109)	(1.101)	(1.123)
Packaged Meat	0.088	-0.035	0.135	0.084
	(0.412)	(0.410)	(0.409)	(0.414)
Observations	67,197	67,197	67,197	67,197

Table 4-4. Parallel Trends Tests: Event Study Estimates for Households' Monthly Spending

	Total Spent	Health & Beauty	Alcohol	Non-Food Grocery	General Merchandise	All Food	Frozen Foods	Dairy	Deli	Packaged Meat
2004 X Treatment	11.114	5.501*	0.652	-1.494	3.347	3.107	-5.308**	-1.363	0.642	-0.956
	(23.139)	(3.223)	(1.567)	(2.430)	(4.182)	(18.216)	(2.157)	(1.636)	(2.031)	(0.841)
2005 X Treatment	-0.422	5.015*	-0.791	-1.633	0.788	-3.800	-2.830	-2.082	0.666	-1.121
	(21.430)	(2.933)	(1.836)	(2.101)	(2.793)	(17.735)	(2.212)	(1.632)	(2.198)	(0.863)
2006 X Treatment	6.420	4.945*	-2.403	-1.746	-4.524	10.149	0.673	-0.930	1.669	-0.044
	(21.132)	(2.633)	(1.708)	(2.071)	(2.769)	(17.899)	(2.005)	(1.421)	(1.948)	(0.776)
2007 X Treatment	-27.961	0.298	-0.550	-2.821	-5.350***	-19.538	-1.660	-2.218*	0.316	-0.766
	(18.011)	(2.003)	(1.239)	(1.747)	(1.824)	(15.709)	(1.770)	(1.191)	(1.742)	(0.596)
2008 X Treatment	-15.502	0.761	-1.260	-0.279	-0.645	-14.079	-1.386	-1.334	-0.138	-1.433**
	(17.417)	(1.648)	(1.037)	(1.836)	(2.655)	(14.387)	(1.569)	(1.028)	(1.298)	(0.610)
2010 X Treatment	22.065	5.283*	1.531	1.640	-0.866	14.478	1.018	0.482	-0.789	-0.337
	(18.177)	(3.037)	(1.235)	(1.739)	(2.648)	(14.864)	(1.643)	(0.957)	(1.393)	(0.584)
2011 X Treatment	-1.885	1.052	1.633	-1.959	-0.315	-2.296	1.375	0.391	0.947	-0.818
	(22.614)	(3.978)	(1.409)	(2.229)	(2.772)	(18.683)	(2.197)	(1.250)	(2.522)	(0.649)
2012 X Treatment	26.512	-1.585	4.536***	-2.166	4.848	20.879	1.273	0.624	7.791	-0.522
	(24.838)	(5.791)	(1.631)	(2.301)	(4.121)	(19.544)	(2.596)	(1.226)	(5.755)	(0.832)
2013 X Treatment	52.114*	-0.584	4.613**	-0.363	8.585	39.863*	3.640	1.295	6.513	-0.381
	(29.719)	(5.016)	(1.816)	(2.465)	(8.768)	(20.509)	(2.683)	(1.590)	(4.729)	(0.983)
Pre-2010 Terms = 0 (p-value)	0.344	0.206	0.365	0.702	0.006	0.419	0.048	0.457	0.480	0.178

Notes: Author's calculations based on Nielsen 2004-2013; 2009 is omitted as the base year. Treatment group is defined as householders in which all members are below age 26. Control group is defined as households in which all members are aged 27 to 31. All outcomes are measured in dollars spent per month, adjusted for inflation. Each column represented a different regression. Cells display coefficients on interaction of treatment and each year. All regressions also control for treatment indicator, race/ethnicity, marital status, age and educational attainment of each householder, month-year fixed effects, and household fixed effects. Standard errors are clustered at the household level. The last row displays the p-value on the F test that all pre-2010 terms jointly equal 0.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

5 Health Insurance Expansions and Racial Disparities in Cancer Outcomes[34]

Abstract

Cancer is a leading cause of death among non-elderly Americans, and there exist large racial and ethnic disparities in cancer detection and mortality. This study assessed the extent to which insurance expansions facilitated by the Affordable Care Act reduced racial/ethnic disparities in cancer screening and detection. We used nationally-representative survey data on preventive care and cancer registry data on diagnoses to study changes in states that did and did not expand Medicaid, before and after the implementation of the expansion. We found that the Medicaid expansion had no detectable effect on cancer screenings for the overall population or for any specific race. Our results also suggest that the incidence of early stage diagnoses increased by a statistically significant 4 percent for Whites ($p<0.05$) and by 22 percent for Hispanics ($p<0.10$); there was no detectable change for Blacks or other non-Hispanic races.

[34] This work is joint with Lindsay Sabik (University of Pittsburgh) and Kosali Simon (Indiana University and National Bureau of Economic Research). I am grateful to the Horowitz Foundation for Social Policy for their support of this research.

5.1 Introduction

Cancer is responsible for 22 percent of all deaths in the United States, making it the second leading cause of death in the country, behind only heart disease. There exist large disparities in cancer detection, treatment, and mortality by race and ethnicity: Black and Hispanic cancer patients are on average diagnosed at later stages than White patients, and mortality rates are higher for non-Whites. One reason for these disparities may be lack of health insurance. Non-White people are less likely to be insured, and uninsurance is a major financial barrier to cancer screening, early diagnosis, and treatment.

One of the goals of the Affordable Care Act (ACA) was to reduce racial/ethnic disparities in cancer outcomes by expanding health insurance. Under the ACA, states have the option to extend Medicaid coverage to low-income people below 138 percent of the federal poverty level (FPL). To date, 31 states and DC have opted to expand Medicaid. We exploited this variation in Medicaid expansion across states and over time to estimate the causal effect of Medicaid on racial/ethnic disparities in cancer diagnosis.

Previous studies have shown that the Medicaid expansion significantly reduced racial/ethnic disparities in insurance coverage (Buchmueller et al., 2016). There is high interest in understanding the extent to which this new coverage impacts health outcomes for people with cancer. Our key findings were that the Medicaid expansion had no detectable effect on cancer screenings for the overall population or for any specific race. We found that the incidence of early stage diagnoses increased by a statistically significant 4 percent for Whites ($p<0.05$) and by 22 percent for Hispanics ($p<0.10$); there was no detectable change for Blacks or other non-Hispanic races. Our analysis informs policymakers of the extent to which public health insurance expansions can reduce racial/ethnic disparities in cancer screening and detection.

5.2 Conceptual Model

It has been well-established that the Medicaid expansions facilitated by the ACA led to large increases in insurance coverage among the low-income, non-elderly population (C. Courtemanche et al., 2018; Frean et al., 2017). There are multiple ways in which this increased insurance coverage may affect cancer outcomes. One scenario is that newly-insured people now have increased access to care and are more likely to go to the doctor for recommended screenings. This may help catch cancer in its early stages. This group of people may have eventually been diagnosed with cancer even if they had remained uninsured, but likely it would have been caught in later stages.

Another potential scenario is abetted by adverse selection. A person who feels some symptoms or has been recently diagnosed with cancer can now procure insurance without any barriers due to pre-existing conditions. Under this scenario, it would be those who are more likely to have cancer who seek insurance. If this scenario is true, there would be an even larger number of people who receive care under the ACA than what would be yielded by simply multiplying the number of uninsured people by the probability that someone has cancer.

5.3 Data

Our primary data sources were the 2010 to 2016 Behavioral Risk Factors Surveillance System (BRFSS) and the National Cancer Institute's 2010 to 2015 Surveillance, Epidemiology, and End Results (SEER) program.

The BRFSS is an annual telephone survey conducted by the Centers of Disease Control and Prevention (CDC) in collaboration with state governments. The survey is nationally representative and collects information on individuals' health behaviors, including preventive

care utilization and cancer screenings. We used BRFSS data to assess the effect of the Medicaid expansions on the probability of receiving recommended cancer screenings. This dataset has been used in past studies to assess the effect of insurance expansions on cancer screening (Barbaresco et al., 2015; Bitler & Carpenter, 2016; Sabik & Bradley, 2016; K. Simon et al., 2017).

Although the BRFSS has a full sample size of nearly 500,000 each year, we restricted our sample to respondents that were aged 19 to 64, had no children below the age of 18, and had household incomes below 100% of the FPL; i.e. the group most targeted by the Medicaid expansion. The exact sample size differed for each outcome because certain cancer screenings are relevant only for specific genders and age groups. Our outcomes of interest for the BRFSS analysis included: whether the respondent received a clinical breast exam in the past year (restricted to women over age 21), whether the respondent received a Pap test in the past year (restricted to women over age 21), whether the respondent received a mammogram in the past year (restricted to women over age 50), and whether the respondent received a colonoscopy in the past year (restricted to men over age 50).

We used the SEER data to assess the effect of the Medicaid expansions on county-level cancer diagnosis rates. The SEER reports information on all patients with cancer in participating areas in the United States, including patient demographics, county of residence, type of cancer, stage of diagnosis, and type of treatment. Although only 13 states participate in SEER (9 expansion states and 4 non-expansion states), the data covers 28% of the US population. The SEER has been used in past studies to study the effect of insurance expansions on cancer detection (Soni, Simon, Cawley, & Sabik, 2018). This study found that ACA Medicaid expansions led to a 3 percent increase in the incidence of total cancer diagnoses in the non-

elderly population and a 6 percent increase in early-stage diagnoses. However, this study did not study potential heterogeneous effects by race/ethnicity.

We augmented the SEER data with county-level uninsurance rates and demographic information from the Census Bureau and county-level cancer mortality rates from the National Vital Statistics System.

The original number of diagnoses reported in SEER from 2010 to 2015 was about 2.8 million. We limited the sample to first-time diagnoses for people aged 19 to 64; we further eliminated those whose county of residence or race/ethnicity information was missing. After making these restrictions, the dataset consisted of about 1.1 million diagnoses. For each race/ethnicity category in each county and in each year, we divided the number of diagnoses in the county by the population to construct county-level diagnosis rates for each race/ethnicity category. So our final analytical sample size was N=14,688 race-county-year observations (6 years X 612 counties X 4 race/ethnicity categories).[35]

The outcomes of interest for the SEER analysis included: county-level cancer diagnosis rate (number of diagnoses that year per 100,000 population of the county) for all diagnoses, early-stage diagnoses, late-stage diagnoses, and unknown stage diagnoses. We defined early-stage diagnoses as those that were in situ, local, or regional by direct extension only. Late-stage diagnoses were those that were regional with only lymph nodes involved, regional with lymph nodes involved and by direct extension, regional not otherwise specified, and distant. Diagnoses

[35] As noted in the tables, the exact sample size is slightly smaller than 14,688 because some counties reported 0 population for certain race/ethnicity categories in certain years. Specifically, in Harding County, New Mexico, there were 0 non-Hispanic others in the years 2012 through 2015. In Piute County, Utah, there were 0 non-Hispanic Blacks in the year 2010. In Alaska, only the American Indian/Alaska Native populations are covered by SEER, so the non-Hispanic White and non-Hispanic Black populations are both 0 in the years 2010-15. The outcome variable was undefined for these observations with missing populations and so they were automatically dropped from analysis.

for which the stage was missing, unknown, or specified as unstaged were included in the unknown stage category.

For all of our analyses, we first assessed the impact of the Medicaid expansion for the entire sample and then separately for each racial/ethnic group to assess whether the Medicaid expansion closed disparities. We stratified our sample into four racial/ethnic groups: non-Hispanic Whites, non-Hispanic Blacks, non-Hispanic other race, and Hispanics.

5.4 Methods

We estimated difference-in-differences (DD) models, comparing changes among people in Medicaid expansion states versus non-expansion states, before and after the implementation of the expansion. Our first set of analyses used BRFSS data to examine the expansion's impact on the probability of receiving the four recommend cancer screenings described in the Data section above. Each outcome was coded as a binary variable: 1 if the respondent received the screening in the past year and 0 if they did not. For each outcome, we estimated the following model:

$$Y_{ist} = \alpha + \beta(Treatment_s * Post_t) + \gamma \boldsymbol{X}_{ist} + \theta UnempRate_{st} + \theta_s + \tau_t + \varepsilon$$

Equation 1

where Y_{ist} represents a cancer screening outcome for individual *i* living in state *s* at time *t*, expressed as a quarter/year combination; $Treatment_s$ is a binary variable equal to 1 if the respondent lives in an expansion state; $Post_t$ is a binary variable equal to 1 if the time period is after the policy implementation (i.e. 2014 or later for most states); $\boldsymbol{X}_{ist}$ is a vector of individual control variables that include age, sex, educational attainment, marital status, employment status, household size, whether the household was part of the cell phone sample; $UnempRate_{st}$ measures the state unemployment rate in a given time period; θ_s represents state fixed effects;

and τ_t represents quarter/year fixed effects. Standard errors were clustered by state, and all regressions accounted for BRFSS sampling weights. Although the outcomes were binary, we estimated linear probability models because they generally give reliable estimates of average effects (Angrist & Pischke, 2009). The β term is our key coefficient of interest. It captures the treatment effect of Medicaid expansion on the outcome of interest.

Our second set of analyses used the 2010-15 SEER data to examine the expansion's impact on county-level cancer diagnosis rates. The data was at the county level, not the individual level. We estimated the following equation for each outcome variable:

$$Y_{cst} = \alpha + \beta(Treatment_s * Post_t) + \gamma \boldsymbol{X}_{cst} + \theta_s + \tau_t + \varepsilon$$

Equation 2

where Y_{cst} represents a cancer diagnosis rate for county *c* in state *s* at year *t*; $Treatment_s$ is a binary variable equal to 1 if the county is in an expansion state; $Post_t$ is a binary variable equal to 1 if the time period is after the policy implementation (i.e. 2014 or later for most states); $\boldsymbol{X}_{cst}$ is a vector of county-level control variables that include county unemployment rate, whether the county is rural, percent of county that is female, percent over age 65, and percent in poverty; θ_s represents state fixed effects; and τ_t represents year fixed effects. β represents the causal effect of Medicaid expansion on the outcome of interest, so long as the DD assumptions are satisfied.

5.4.1 Parallel Trends Tests

The key identifying assumption of the DD model is that in the absence of treatment, the expansion states and non-expansion states would have trended similarly. We tested the validity of this assumption by first visually analyzing trends in expansion and non-expansion states

before 2014 and evaluating the extent to which they trended similarly in pre-expansion time period. We then formalized the parallel trends tests by estimating models similar to those described in Equations 1 and 2, but we replaced the $Treatment_s * Post_t$ term with a vector of interactions between $Treatment_s$ and an indicator for each year, excluding 2013 as the base year. If we were to find coefficients that are insignificant and close to zero for the pre-2014 interaction terms, it would increase our confidence that expansion and non-expansion states trended similarly before the expansion.

5.4.2 Sensitivity Analyses

We conducted a set of analyses with county-level demographic control variables (percent of county that is female, percent of county that is above 65, rurality of the county). However, some of these variables were missing for Alaska (county 900). We therefore estimated a set of models that did not include these demographic control variables in order to include the Alaskan counties in our analysis.

In constructing county-level cancer diagnosis rates, we realized that not all race/ethnicity categories were represented in all counties in all years. If, for example, the original SEER database did not report a single diagnosis by a Hispanic person in Jones County, Iowa in 2013, then we assumed that 0 Hispanic persons in Jones County had cancer that year; therefore, the outcome variable for the Hispanic-Jones County, IA-2013 observation was imputed as 0. We estimated a set of models in which we excluded these imputed zeros. Our sample size for this non-imputed analysis was 9,942 county-year-race/ethnicity observations.

Another potential concern may be potential non-random shifts in racial/ethnic composition of counties over time. To address this concern, we estimated another set of models in which we used a fixed base population as the denominator for the outcome variables.

5.5 Results

Figure 5-1 displays trends in each outcome over time, separately for expansion states and non-expansion states.

Table 5-1 shows the DD for the effect of Medicaid expansion on each cancer screening outcome for the overall sample, as well as stratified by race/ethnicity. We found that Medicaid expansion did not significantly change the probability of receiving breast exams, Pap tests, mammograms, or colonoscopies for the overall population. Even when we stratified the sample by race/ethnicity, we found that with the exception of the Hispanic population, there were no detectable impacts of the expansions on cancer screenings. The probability of receiving a clinical breast exam increased for Hispanic women by 0.11 percentage points ($p<0.10$). This represented a 17 percent increase compared to pre-expansion levels.

Table 5-2 displays the DD estimates for each cancer diagnosis rate outcome for the overall sample, as well as stratified by race/ethnicity. We found that early stage diagnoses increased by a statistically significant 10.1 diagnoses per 100,000 population for Whites ($p<0.05$) and by 17.9 per 100,000 population for Hispanics ($p<0.10$). Relative to pre-expansion levels, this represented a 3.9 percent increase for Whites and a 22 percent increase for Hispanics. There was no detectable change for Blacks or other non-Hispanic races. There was no significant impact of Medicaid expansion on late stage diagnoses or unstaged diagnoses for the overall population or any particularly racial/ethnic group.

Results for the parallel trends test are displayed in Appendix Table 5- 1.

5.6 Discussion

These results fill a critical void in our understanding of Medicaid's role in reducing racial/ethnic disparities in cancer screening and diagnosis. We found that the Medicaid expansion had no detectable effect on cancer screenings for the overall population or for any specific race. However, the incidence of early stage diagnoses increased by a statistically significant 4 percent for Whites ($p<0.05$) and by 22 percent for Hispanics ($p<0.10$); there was no detectable change on Blacks or other non-Hispanic races. The large impact that we find for Hispanics is in line with other work showing that insurance expansions have particularly benefited Hispanics with cancer (Sabik, Tarazi, Hochhalter, Dahman, & Bradley, 2018). Sabik et al. (2018) found increases in cervical screening after pre-ACA expansions to non-elderly adults for Hispanics.

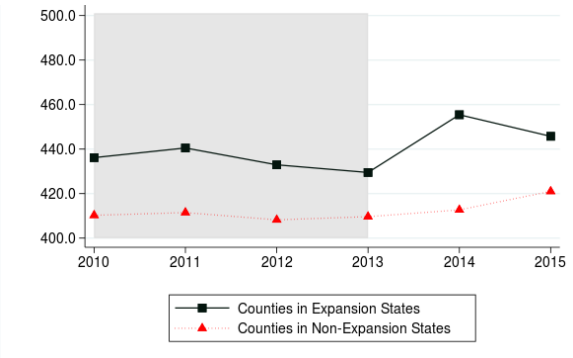
There are several possible explanations for the null finding on cancer screenings. First, we have only three years of post-expansion data and it may take more time for impacts to manifest (as with mammography in Massachusetts). Second, our data may be underpowered to detect effects in small racial/ethnic groups. Third, in spite of the new coverage, there may other barriers to accessing preventive care. For example, if there is general lack of awareness about the importance of cancer screenings in this population, or if there is a short supply of providers who accept Medicaid insurance.

Figures

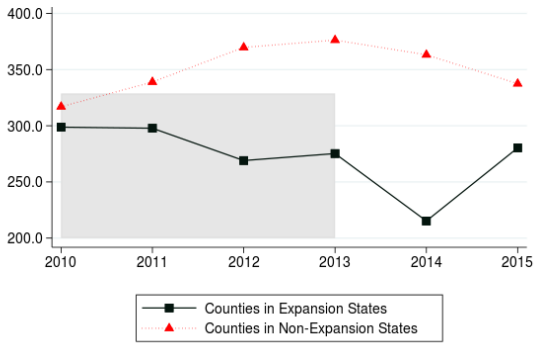
Figure 5-1. Trends Graphs for Each Race

Panel A: All Diagnoses Per 100,000 Population

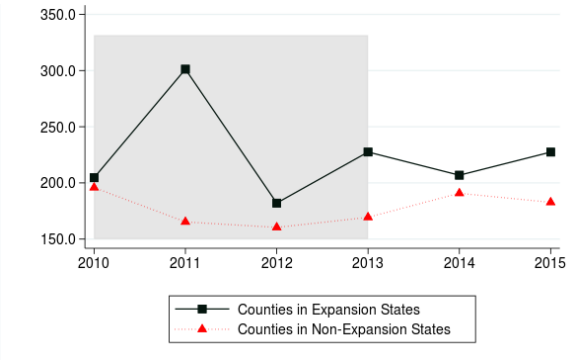
White, non-Hispanic



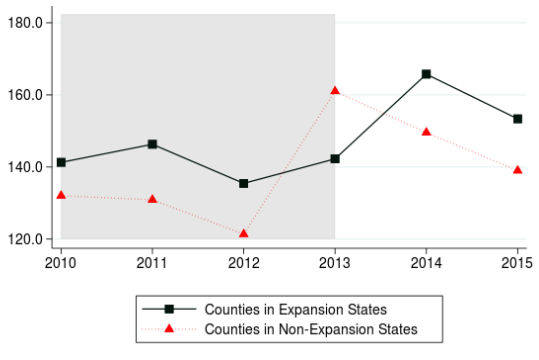
Black, non-Hispanic



Other, non-Hispanic

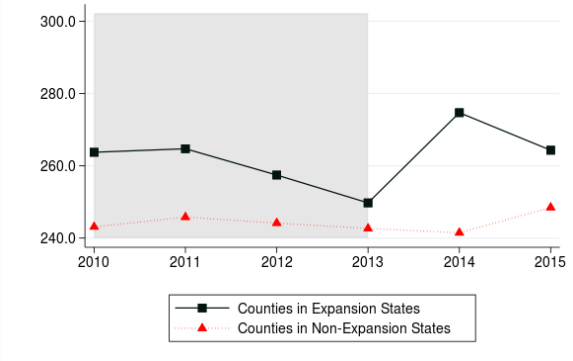


Hispanic

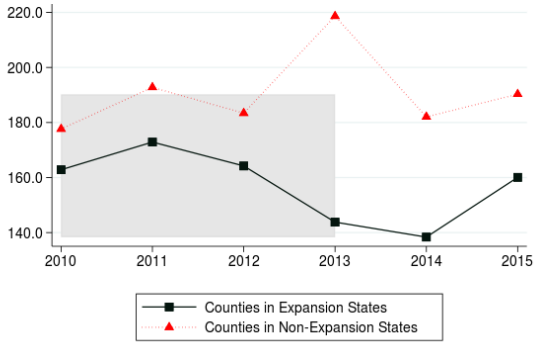


Panel B: Early Stage Diagnoses Per 100,000 Population

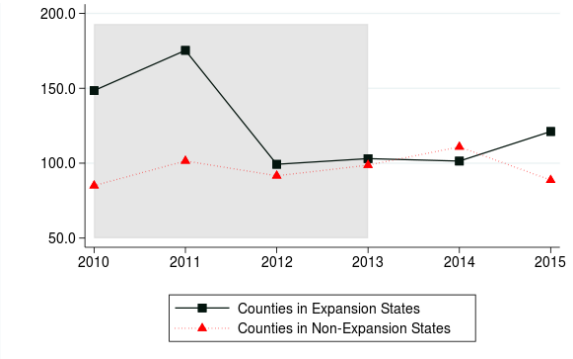
White, non-Hispanic



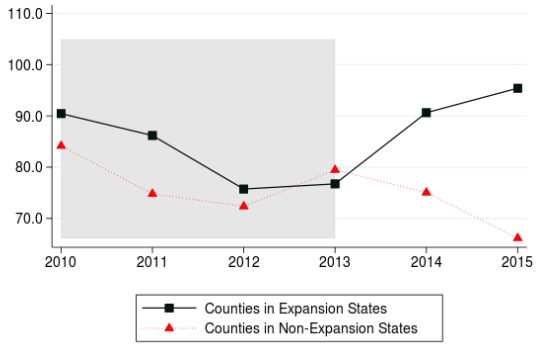
Black, non-Hispanic



Other, non-Hispanic

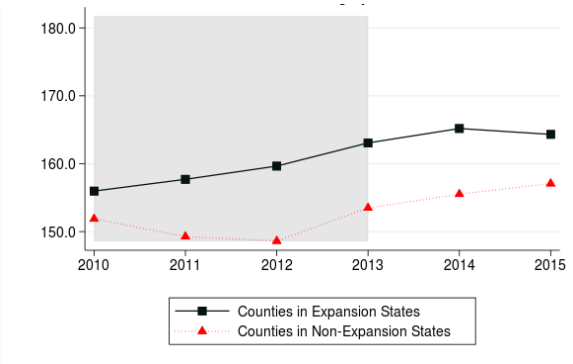


Hispanic

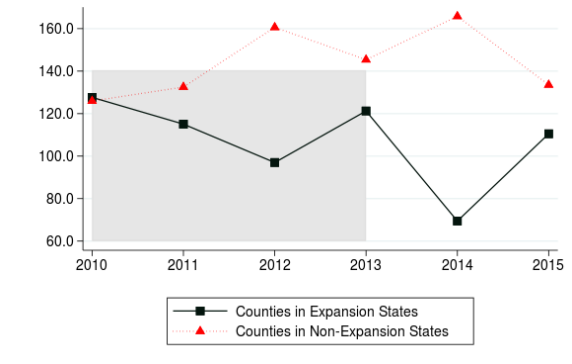


Panel C: Late Stage Diagnoses Per 100,000 Population

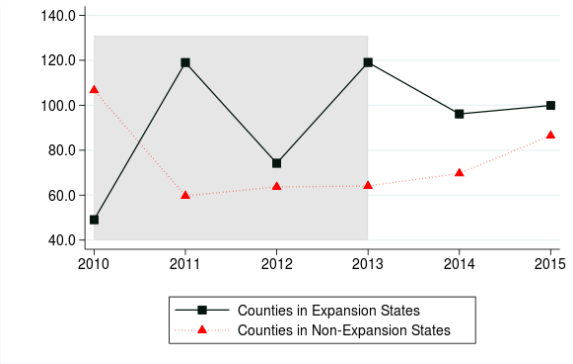
White, non-Hispanic



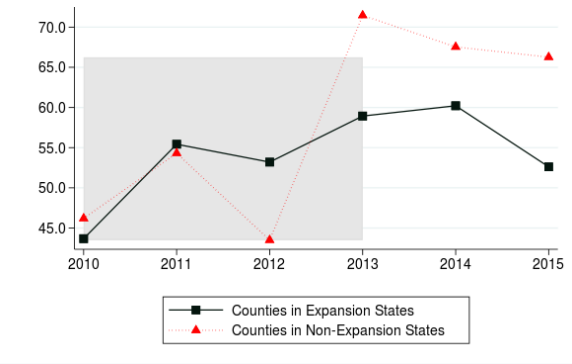
Black, non-Hispanic



Other, non-Hispanic

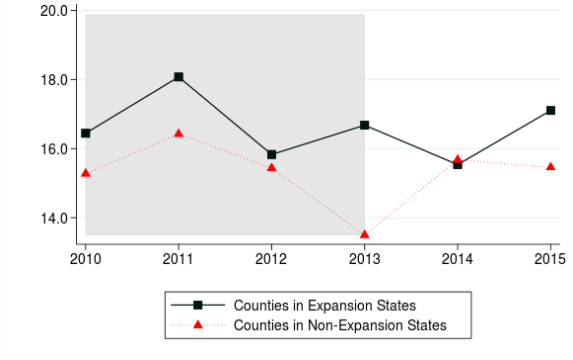


Hispanic

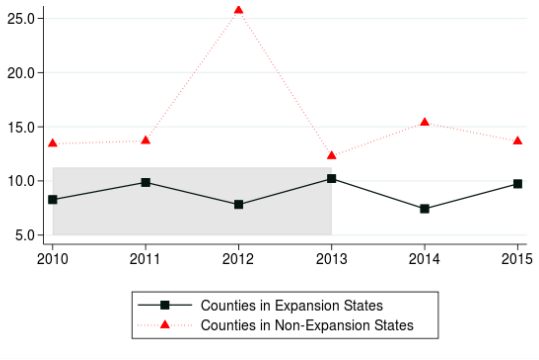


Panel D: Unstaged Diagnoses Per 100,000 Population

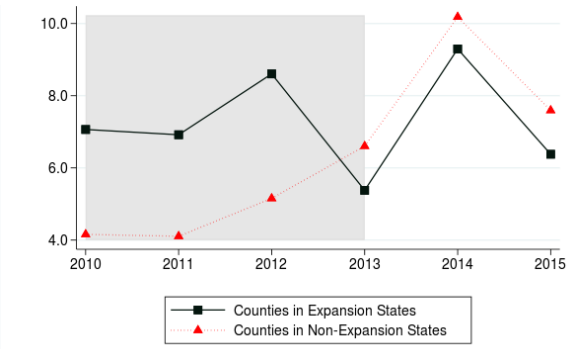
White, non-Hispanic



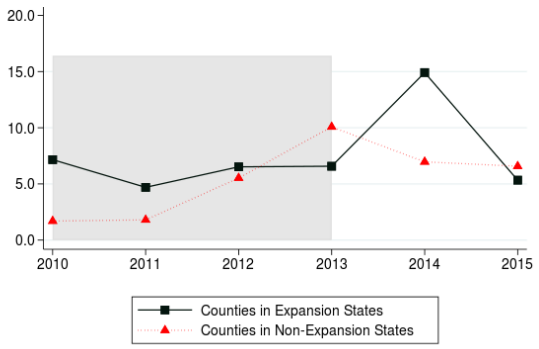
Black, non-Hispanic



Other, non-Hispanic



Hispanic



Source: Authors' calculations based on SEER 2010 to 2015.

Tables

Table 5-1. Impact of Medicaid Expansion on Cancer Screening Rates by Race/Ethnicity – Difference-in-Differences Estimates

	All Races	White, non-Hispanic	Black, non-Hispanic	Other race, non-Hispanic	Hispanic
Clinical breast exam	-0.005	-0.020	-0.040	-0.170	0.112*
	(0.025)	(0.040)	(0.075)	(0.123)	(0.059)
	μ=0.56	μ=0.51	μ=0.61	μ=0.51	μ=0.65
	N=17,046	N=10,537	N=3,197	N=1,273	N=1,600
Pap test	-0.007	0.005	0.019	-0.132	-0.022
	(0.026)	(0.028)	(0.060)	(0.102)	(0.053)
	μ=0.44	μ=0.38	μ=0.53	μ=0.38	μ=0.53
	N=23,254	N=14,359	N=4,300	N=1,564	N=2,268
Mammogram	0.019	0.040	-0.106	-0.014	-0.004
	(0.024)	(0.040)	(0.067)	(0.118)	(0.107)
	μ=0.55	μ=0.49	μ=0.61	μ=0.48	μ=0.63
	N=15,752	N=9,588	N=2,943	N=1,014	N=1,636
Colonoscopy	0.020	0.011	0.008	0.122	-0.066
	(0.023)	(0.042)	(0.054)	(0.109)	(0.063)
	μ=0.41	μ=0.43	μ=0.41	μ=0.30	μ=0.35
	N=11,691	N=7,475	N=1,775	N=877	N=965

Source: Authors' calculations based on BRFSS 2010 to 2016. Sample was restricted to include non-elderly childless adults with household income below 100% FPL. State-clustered standard errors are in parentheses. All regressions also control for age, sex, educational attainment, marital status, employment status, household size, whether the household was part of the cell phone sample, state unemployment rate, state fixed effects, and year fixed effects. Data is adjusted by BRFSS sample weights. Pre-expansion means for expansion states (μ) are displayed below each DD estimate.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5-2. Impact of Medicaid Expansion on Cancer Diagnosis Rates by Race/Ethnicity – Difference-in-Differences Estimates

	All Races	White, non-Hispanic	Black, non-Hispanic	Other race, non-Hispanic	Hispanic
Total Diagnoses per 100,000 Population	-10.58 (13.99) μ=272.6	9.53 (6.76) μ=434.8	-37.48 (33.13) μ=285.2	-24.68 (38.78) μ=228.9	10.39 (14.24) μ=141.3
Early-Stage Diagnoses per 100,000 Population	-0.57 (10.55) μ=158.4	10.13** (5.12) μ=258.9	-5.01 (23.72) μ=160.9	-25.23 (31.39) μ=131.5	17.90* (10.45) μ=82.3
Late-Stage Diagnoses per 100,000 Population	-9.81 (8.61) μ=104.4	0.26 (4.01) μ=159.1	-33.70 (22.97) μ=115.2	3.59 (23.03) μ=90.3	-9.37 (8.68) μ=52.8
Unknown-Stage Diagnoses per 100,000 Population	-0.20 (1.62) μ=9.8	-0.86 (1.21) μ=16.8	1.23 (4.05) μ=9.0	-3.04 (3.57) μ=7.0	1.87 (3.35) μ=6.2

Source: Authors' calculations based on SEER 2010 to 2015. Sample was restricted to first-time cancer diagnoses for adults aged 19 to 64. N=14,652 county-year observations. All regressions also control for county unemployment rate, whether the county is rural, percent of county that is female, percent that is over age 65, percent in poverty, state fixed effects, and year fixed effects. Standard errors are in parentheses. Pre-expansion means for expansion states (μ) are displayed below each DD estimate.

* p<0.10, ** p<0.05, *** p<0.01

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Appendices for Chapter 1

1-A. Background on Opioids and Other Pain Relief Drugs

Pain relief drugs—which include opioids (also known as narcotics), non-steroidal anti-inflammatory drugs (NSAIDs, such as Ibuprofen), and Acetaminophen (such as Tylenol)—are among the most frequently prescribed therapeutic classes in the United States. These drugs are also known as analgesics, meaning that they produce a reduction in the perception of pain. In 2015, pain relief drugs accounted for nearly 8% of total prescriptions taken by adults in the United States, and 24% of adults used a prescription analgesic at least once during the year. Over the past 15 years, there has been a shift in the type of painkillers prescribed: opioids accounted for only 38 percent of total painkiller prescriptions in 2000 but 51 percent of prescriptions by 2015 (author's calculations based on Medical Expenditure Panel Survey). This is a worrisome trend because opioids are not only the strongest pain medications but also pose the highest risk for addiction.

Spending on prescription opioids has grown rapidly over the past 15 years (Appendix Figure 1- 1). Panel A shows that average annual opioid spending was $9 per person in 2000 and more than tripled to $32 per person in 2015. Over the same time period, the share of spending attributable to public sources more than doubled from 24 percent in 2000 to 51 percent in 2015.

Panel B of Appendix Figure 1- 1 shows that this increase was even more pronounced for the elderly population. Among people over age 65, average annual opioid spending increased from $17 per person in 2000 to $63 per person in 2015. Meanwhile, the share of spending attributable to public sources nearly tripled from 24 percent in 2000 to 66 percent in 2015.

How Pain Relief Drugs Work

This section briefly describes the biochemistry of pain and pain relief drugs (Carroll, 2016; Purves et al., 2004). The human brain and nervous system consist of nerve cells called neurons. Neurons communicate with each other by firing electrical signals to release chemical messengers, called neurotransmitters, across the tiny spaces between cells; this process is called neurotransmission. Nerve receptors are located all over the human body and send signals to the brain when they are exposed to certain stimuli, such as temperature. Nociceptors are specialized nerve receptors that only fire when something is causing damage to the body (e.g. if the skin is cut, a muscle is pulled, etc.). Nociceptors are located in skin, organ walls, and within body tissues such as muscles and joints. When the body encounters a noxious stimulus, nociceptors transmit electrical signals to the spinal cord, where neurotransmitters are released to send the signal up to the brain, where it is interpreted as pain. These pain signals are transmitted in a fraction of a second; their purpose is to alert the body to potential harm.

Opioids are effective painkillers because they inhibit the pain signal at multiple steps in the pathway from the nociceptors to the brain. In the brain, opioids cause sedation and alter moods that decrease the emotional response to pain. At the nociceptor level, opioids block the signaling from the nociceptors to secondary neurons. Along the spinal cord, opioids chemically bind to specific opioid receptors on neurons, which decreases the release of neurotransmitters that are trying to communicate the pain signal. This results in less pain experienced by the brain.

The reason human spinal cords have opioid receptors is because the body has a built-in analgesic system that regulates pain signals. The human body produces endogenous opioids, known as endorphins, which bind to neurons and produce pain relief. Opioid drugs bind to opioid receptors in a similar way that endorphins produced by the body do, but with more powerful side effects, such as intense euphoria, severe respiratory depression, sedation, urinary retention, nausea, dizziness, and constipation.

Moreover, opioid medications are associated with tolerance (with time, higher doses are required to get the same level of pain relief) and severe withdrawal symptoms if one stops the drug. This can lead to physical and psychological dependence on the drug. An opioid overdose refers to toxicity due to excessive opioids; an overdose can lead to insufficient breathing, loss of consciousness, and death. Because of the drug's dangerous potential for addiction, opioid sales are controlled by the US Drug Enforcement Authority (DEA).

Since 1970, the DEA has classified certain drugs and other substances, called controlled substances, into five schedules based on risk of abuse or harm. Schedule I drugs, such as heroin, have high risk and no counterbalancing benefit and are banned from medical use. Schedule II drugs have high potential for abuse and can lead to severe psychological or physical dependence; examples include hydromorphone, methadone, meperidine, oxycodone, fentanyl, morphine, opium, and codeine. Schedule III drugs have less potential for abuse but can still lead to moderate/low physical dependence or high psychological dependence. Schedule III opioids include combination products containing less than 16 mg of hydrocodone per dose and less than 90 mg of codeine per dose (for example, Tylenol with Codeine). Schedule IV drugs have low potential for abuse (for example, Tramadol), and Schedule V drugs have even lower potential for abuse (for example, Robitussin AC).

NSAIDs – which include Ibuprofen, Aspirin, and COX-2 inhibitors – work differently from opioids. When cells and tissues are damaged, they prompt the body's COX-1 and COX-2 enzymes to produce chemicals known as prostaglandins. Prostaglandins lower the threshold required for nearby nociceptors to fire (i.e. reduce the body's pain threshold); this results in more pain signals being transmitted to the brain. NSAIDs work by competitively inhibiting production of prostaglandins from the COX enzymes; the drug competes for the binding sites on the COX enzymes. Reduced production of prostaglandins diminishes the intensity of pain signals being sent to the brain, and as a result, the body experiences pain relief. Side effects of long-term NSAID use can include heartburn and stomach ulcers.

Acetaminophen – which includes Tylenol – is another class of commonly-used pain relief drugs, but researchers have not yet determined exactly how the drug works. The physician's directions that come with Acetaminophen prescriptions usually include the note, "Although the analgesic effect of Acetaminophen is well established, the site and mode of action have not been clearly elucidated." Side effects of long-term use of Acetaminophen can include liver damage and trouble passing urine.

The side effects associated with NSAIDs and Acetaminophen are substantially less severe than those of opioids. Moreover, neither NSAIDs nor Acetaminophen share the addictive properties associated with opioids. The Centers for Disease Control and Prevention (CDC) therefore recommends NSAIDs and Acetaminophen as first-line therapies for chronic pain outside of cancer treatment and end-of-life care (Centers for Disease Control and Prevention, 2016). Neither NSAIDs nor Acetaminophen are controlled by the DEA, unless combined with opioids.

Causes of Opioid Growth

Researchers have proposed several possible explanations for the rapid growth of opioids since the late 1990s. One school of thought focuses on the increased demand for opioids. Economic studies point out that certain population cohorts in the United States (particularly middle-aged White men) have experienced relative declines in permanent income in recent decades; this phenomenon may push the struggling cohorts to opioid addiction, suicide, and other "deaths of despair" (Case & Deaton, 2015). Ignorance about the addiction potential of opioids and increased prevalence of physical pain are other potential reasons for growth in the demand for opioids.

Another set of explanations faults the increased supply of opioids. During the 1990s, new attitudes in medicine promoted the treatment of pain as the fifth vital sign and destigmatized the prescription of opioids for non-cancer pain. Meanwhile, drug manufacturers initiated aggressive marketing campaigns for opioids, often funding continuing medical education seminars for physicians and offering other in-kind perks to doctors. When asked about the addictive potential of opioids, sales representatives often pointed to a 1980 study which found that less than 1 percent of patients taking narcotics developed addiction to them (Porter & Jick, 1980); however, that one-paragraph publication was based on a study of hospitalized patients, not those going home with opioid prescriptions. In particular, Purdue Pharmaceutical aggressively marketed its time-release formula of oxycodone – Oxycontin – as a virtually non-addictive pain relief drug. In 2005, Purdue pled guilty to false branding and paid a $634 million fine. Moreover, there was little regulation of pain management clinics ("pill mills"), making prescription opioids even easier to access. On the illicit side, heroin became cheaper and more pure in quality, fueled by

the rampant growth of the black tar heroin from the Xalisco region of Mexico. This eased the transition from prescription to illicit opioids for those who became addicted.

Consequences of Opioid Misuse

One of the most devastating consequences of opioid misuse is the elevated rate of overdose deaths in the United States. By the year 2010, drug overdoses – driven by opioids – became the leading cause of death from injury, surpassing motor vehicle accidents. Panel A of Appendix Figure 1- 2 shows that the number of opioid overdose deaths increased from 8,400 per year in 2000 to 42,200 per year in 2016. Opioid overdoses may be from licit prescription opioids as well as illicit opioids such as heroin and illegally produced fentanyl. According to Panel A, prescription opioids have played an increasingly larger role in overdose deaths over time: prescription opioids were responsible for 52 percent of overdose deaths in 2000 and 77 percent by 2016. However, the underlying mortality data cannot distinguish deaths from pharmaceutical fentanyl and those from illegally produced fentanyl. Therefore, the prescription opioids bar may contain deaths from both prescription and illicit fentanyl.

Panel B of Appendix Figure 1- 2 provides an alternative way to describe the split between prescription and illicit opioid deaths.[36] The "semisynthetic and natural opioids" and the "heroin" bars refer unambiguously to prescription and illicit opioids, respectively. The "synthetic opioids" bar consists of deaths from both prescription and illicit fentanyl. In the year 2016, semisynthetic and natural (prescription) opioids accounted for 34 percent, synthetic (prescription and illicit) accounted for 37 percent, and (illicit) heroin accounted for 29 percent of total opioid

[36] The aggregate numbers in Panel B are slightly higher than those in Panel A because the three categories presented in Panel B are not mutually exclusive; deaths that involve more than one type of opioid are included in every applicable category.

overdose deaths. The most common drugs involved in prescription opioid deaths include methadone, oxycodone, and hydrocodone.

There are several other health and economic consequences of opioid misuse, in addition to deaths from overdose (Quinones, 2015; Temple, 2015). Shared needles increase the incidence of HIV and Hepatitis C. Opioid use by women during pregnancy can lead to neonatal abstinence syndrome: babies develop addiction in the womb and experience withdrawal after birth, leading to conditions such as seizures, breathing problems, and diarrhea. Prescription opioid abuse has also been linked to increased drug diversion, crime, emergency department utilization, and demand for illicit opioids (Council of Economic Advisers, 2017; Jones, 2013; Powell et al., 2017).

Policy Responses to the Opioid Crisis

Reducing prescription opioid misuse is a top public health priority for policymakers at all levels of government, as well as leaders of the private sector. At the federal level, the White House declared the opioid crisis "a national public health emergency under federal law" (White House, 2018). The Centers for Disease Control and Prevention (CDC) has issued new guidelines urging providers to reduce opioid prescribing and substitute toward other non-opioid therapies (Centers for Disease Control and Prevention, 2016). At the state level, 46 state governors have signed a compact promising to take steps to reduce inappropriate opioid prescribing (National Governors Association, 2013); already several states have strengthened prescription drug monitoring programs (PDMPs) and expanded Naloxone access as ways to reduce opioid overdose deaths. Recently, insurance companies have also taken steps: in 2017, 16 major health insurance companies representing 245 million covered lives adopted eight National Principles of Care and pledged to increase access to treatment for substance use disorder (Pellitt, 2017).

Policy responses to the opioid crisis can be classified into two categories. The first set of policies intends to mitigate harm for existing users by increasing access to treatment for opioid use disorder. Pharmacotherapy programs to treat opioid use disorder typically use one of three drugs. (1) Naltrexone is an opioid antagonist; it prevents the effects of opioids (euphoria, pain relief, etc) and decreases the desire to take opioids. (2) Methadone is a synthetic opioid agonist, meaning that it acts as other opioid drugs by binding to opioid receptors. However, unlike other opioids, Methadone stays in the system for up to 59 hours (compared to six hours for normal-release opioids) and does not demand increasing doses every few hours. Methadone can relieve withdrawal symptoms and cravings for other opioids, and is often used as a replacement drug in treatment for opioid addiction. (3) Buprenorphine is a partial opioid agonist that works by occupying opioid receptors but without stimulating a strong euphoric effect associated with other opioids; it can also reduce cravings and withdrawal symptoms. In addition to these three pharmacotherapy programs, another drug can effectively act as an overdose antidote: Naloxone is an opioid antagonist, meaning that it blocks opioid receptors by binding to the receptors in place of opioid drugs, and can reverse an overdose. Recent policies attempt to increase access to these drugs by making Naloxone available over the counters, increasing waivers for physicians to prescribe Buprenorphine, and expanding access to addiction cessation therapy through insurance expansions.

The second category of policies focuses on preventing future misuse by restricting access to prescription opioids. In recent years, many states have strengthened their prescription drug monitoring programs (PDMPs), databases in which retail pharmacists enter information about controlled substance prescriptions. Providers can access PDMP databases before providing a patient with a prescription to ensure that the patient is not doctor shopping. Some studies have

found that mandatory access PDMP laws reduce opioid prescribing (Bao et al., 2016; Buchmueller & Carey, 2018; Grecu, Dave, & Saffer, 2019; Patrick, Fry, Jones, & Buntin, 2016; Radakrishnan, 2014). Other policies include the increased regulation of pain management clinics, the promotion of abuse-deterrent opioid formulations, tougher prescriber guidelines from the CDC, and 7-day limits on initial opioid prescriptions for opioid-naïve patients prescribed the drugs to treat acute pain.

1-B. Review of Literature on Price Elasticities of Demand for Prescription Drugs

Appendix Table 1- 1 summarizes methods, data sources, and results of 31 studies that estimate price elasticities of demand for prescription drugs. The empirical methods used in these studies exploit exogenous changes in out-of-pocket (OOP) drug prices, such as those caused by the introduction of Medicare Part D in 2006, entering the Part D coverage gap (donut hole), changes in benefit design of private insurance, the RAND Health Insurance Experiment, and the introduction of drug copayments in the United Kindgom's National Health Service. Within each category, studies are sorted by year of publication. It should be noted that the studies listed in Appendix Table 1- 1 include only those that provide estimates for the policy's impact on OOP costs as well as drug utilization and are thus able to calculate implied elasticities.

There exist a large number of studies that assess the impact of prescription drug coverage or other policy changes on utilization alone; these are not included in Appendix Table 1- 1. Notable papers in this category include a study that uses panel data from the Health and Retirement Study Prescription Drug Study and finds that gaining prescription drug coverage through Part D leads to a 15 percent increase in the number of prescription drugs taken (Engelhardt, 2011). Another paper uses an instrumental variables approach to assess the impact of prescription drug coverage on drug utilization; the authors use data from the Medicare Current

Beneficiary Survey and find that drug coverage through Part D increases drug utilization by 30 percent (Kaestner & Khan, 2012). Neither of these studies calculates implied elasticities of prescription drugs.

Appendix Table 1- 1. Studies that Estimate Price Elasticities of Demand for Prescription Drugs

Paper	Methods	Results
Studies Based on the Introduction of Medicare Part D		
Lichtenberg & Sun. (2007). "The Impact of Medicare Part D on Prescription Drug Use by the Elderly." *Health Affairs*.	The authors use a sample of the 2004-06 Walgreens pharmacy data (N=585 million prescriptions) and estimate DD models to compare drug use (measured in units of days of therapy) and OOP costs (per days of therapy) among those aged 65 and older to those under 65.	Part D reduced OOP costs by 18.4 percent and increased quantity by 12.8 percent. The elasticity of demand for prescription drugs is **-0.70**.
Yin et al. (2008). "The Effect of the Medicare Part D Prescription Benefit on Drug Utilization and Expenditures." *Annals of Internal Medicine.*	DD models and a sample of pharmacy data from Walgreens for the years 2004-07 are used to compare prescription utilization (measured in pill-days) and out-of-pocket expenditures for those aged 66 to 79 with a control group aged 60 to 63 (N=177,311 individuals), before and after January 2006.	From January to May 2006, Part D increased use of medications by 1.1 percent and decreased OOP costs by 8.8 percent (implied elasticity of **-0.13**). From June 2006 to April 2007, utilization increased 5.9 percent and OOP costs decreased 13.1 percent (implied elasticity of **-0.45**). The effect over the earlier period represents the effect of increasing enrollment and the selection effect of early enrollees (who were unhealthier on average) than late enrollees. The effect over the later period represents the steady-state effect of Part D.
Ketcham & Simon. (2008). "Medicare Part D's Effect on Elderly Drug Cost and Utilization." *American Journal of Managed Care.*	The authors use 2005-07 pharmacy records from Wolters Kluwer Health (N=1.4 billion prescription records filled by 34 million patients aged 58 and older) and estimate DD models comparing individuals 66 and older vs those aged 58-64, before and after January 2006. Outcomes include OOP cost per day's supply of a medication, the days of medication supplied per capita, and the number of individuals filling prescriptions.	Part D reduced OOP cost per day's supplied of medication by 21.7 percent and increased use of prescription drugs by 4.7%, implying a price elasticity of demand of **-0.22**.
*Schneeweiss et al. (2009).	Using 2005-06 pharmacy claims data, the authors assess changes in drug utilization (measured by daily doses of medication) before and after 2006 among a group of previously drug	Utilization increased by between 3 and 37 percent, and OOP spending decreased by between 37 and 58 percent, depending on the drug class. The demand elasticities are **-0.35** for

	uninsured elderly individuals (N=114,766). The authors impute insurance status based on medication costs and patients' OOP spending. However, this study only examines utilization of four essential drug classes.	warfarin, **-0.44** for statins and clopidogrel, and **-0.76** for PPIs.
Duggan & Scott-Morton. (2010). "The Effect of Medicare Part D on Pharmaceutical Prices and Utilization." *American Economic Review.*	This study investigates the effect of Part D on price and utilization of branded drugs. The empirical strategy exploits variation across drugs in their pre-2006 Medicare market shares and compares growth in drug prices for drugs that are more reliant on Medicare customers with drugs that are less reliant on Medicare customers. The authors use MEPS data to calculate Medicare market shares and 2001 to 2006 IMS Health data to obtain data on price and utilization outcomes.	In addition to reducing the share of the drug price paid by the patient, Part D also reduced gross prices of prescription drugs about 20 percent lower than they otherwise would have been. Prices of brand-name drugs with close substitutes decreased because insurers could structure their formularies to drive demand toward generics and thus had substantial bargaining power with pharmaceutical companies. The study estimates a price elasticity of **-0.38** for prescription drugs.
Liu et al. (2011). "The Impact of Medicare Part D on Out-of-Pocket Costs for Prescription Drugs, Medication Utilization, Health Resource Utilization, and Preference-Based Health Utility." *Health Services Research.*	The authors use DD models and the MEPS 2005-06 panel data to estimate price and utilization outcomes (measured in units of prescriptions) for those aged 65 and older with those aged 55 to 63 (N=1,105), before and after January 2006. The study sample excludes those with Tricare, VA, Medicaid, other state and government subsidies, those with income <125% FPL, and those with cognitive limitations.	OOP costs for prescription drugs increased by $180 (or 21.1 percent from 2005 levels) and utilization increased by 2.05 prescriptions (or 9.3 percent) per patient year. The implied elasticity is **-0.44**.
Studies that Exploit the Medicare Part D Coverage Gap (Donut Hole)		
Einav, Finkelstein, & Schrimpf. (2015). "The Response of Drug Expenditure to Nonlinear Contract Design: Evidence from Medicare Part D." *Quarterly Journal of Economics.*	The authors use administrative data of 2007-09 Part D formularies and Part D claims (N=3.9 million beneficiary years) to study the response of drug use to the future out-of-pocket price. They exploit variation in beneficiaries' birth months, which generates variation in contract duration in their first year of eligibility, which in turn predicts their probability of reaching the Part D coverage gap.	The implied elasticity of drug spending with respect to price ranges from **-0.75 to -0.5**, depending on the magnitude of the price change.
Aron-Dine et al. (2015). "Moral Hazard in Health Insurance: Do Dynamic Incentives Matter?" *Review of Economics and Statistics.*	Part D claims data for the years 2007-09 (N=138,000 individuals) are used to analyze how individuals' initial drug utilization responds to future OOP prices. The authors take advantage of the fact that enrollees can enroll in Medicare at age 65 but their plan resets on January 1 regardless of the month in which they enroll. They exploit variation in birth month, which predicts enrollees'	The implied elasticity of initial prescription drug claims with respect to the future price is **-0.25.**

	probability of reaching the coverage gap.	
Kaplan & Zhang. (2016). "Anticipatory Behavior in Response to Medicare Part D's Coverage Gap." *Health Economics.*	The authors examine whether individuals anticipate copayment changes in their Part D plans and adjust consumption in advance. They exploit variation in beneficiaries' birth months, which generates variation in contract duration in their first year of eligibility, which in turn predicts their probability of reaching the Part D coverage gap. They also use DD models to compare their main study group with those who receive low-income subsidies and do not face the coverage gap.	The implied elasticity of drug utilization (measured as number of prescriptions) with respect to future price ranges from **-0.2 to -05.**
*Einav, Finkelstein, & Polyakova. (2018). "Private Provision of Social Insurance: Drug-Specific Price Elasticities and Cost Sharing in Medicare Part D." *AEJ: Economic Policy.*	This study exploits sharp increases in OOP prices created by the Part D coverage gap to estimate price elasticities of demand across more than 150 drugs and more than 100 therapeutic classes. The authors use administrative data of Part D formularies and Part D claims from 2007 to 2011 (N=6.5 million beneficiary-years).	There is considerable heterogeneity in the price elasticity of demand across products; the average elasticity of the probability of any December purchase with respect to OOP price is **-0.24** and standard deviation is 0.49. The elasticity of opiate agonists is **-0.04**. For NSAIDs, the elasticity is **-0.33** for non-maintenance NSAIDs, **+0.07** for maintenance NSAIDs, and **-0.15** for other NSAIDs.

Studies that Exploit Cost-Sharing Changes in non-Medicare Part D Settings

Harris, Stergachis, & Ried. (1990). "The Effect of Drug Copayments on Utilization and Cost of Pharmaceuticals in a Health Maintenance Organization." *Medical Care.*	Exploiting the 1983 implementation of a cost-sharing prescription drug plan in Washington, the authors analyze the effect of copay increases on the number of prescriptions utilized.	A $1.50 copay led to a 10.7 percent decrease in the number of prescriptions. Increasing the copay from $1.50 to $5 led to an additional 10.6 percent decrease. The price elasticity of demand for drugs is **-0.05 to -0.08**.
Smith. (1993). "The Effects of Copayments and Generic Substitution on the Use and Costs of Prescription Drugs." *Inquiry.*	This study assesses the effect of increases in drug copayments from $2 to $5 for a set of employer groups covered by a national managed care company.	The price elasticity of demand is **-0.10**. Physicians compensated for the increased price to consumers by prescribing larger amounts per prescription.
Coulson & Stuart. (1995). "Insurance Choice and the Demand for Prescription Drugs." *Southern Economic Journal.*	The authors use panel data based on a survey of 4,066 elderly Pennsylvanians enrolled in Medicare. They study the effect of Pennsylvania's PACE program, which provides subsidized drug coverage for elderly Medicare beneficiaries and imposes a $4 copayment per prescription.	The average subsidy was 82.2 percent, and the quantity of prescriptions purchased increased 27.6 percent. The own-price elasticity of drugs is thus **-0.34**.
*Ellison et al. (1997). "Characteristics of Demand for Pharmaceutical Products: An Examination of Four	The authors model demand for four cephalosporins using a multistage budgeting approach. Three of the drugs lost patent protection during this period, which enables the study of generic	Own-price elasticities of the generic versions of the drugs are relatively larger and range from **-1.07 to -4.34**. Own-price elasticities of demand for the branded version of the drugs are

Cephalosporins." *RAND Journal of Economics.*	substitution.	smaller and range from **-0.39 to -2.97**. Cross-price elasticities between branded and generic versions of each drug are positive.
Johnson et al. (1997). "The Effect of Increased Prescription Drug Cost-Sharing on Medical Care Utilization and Expenses of Elderly Health Maintenance Organization Members." *Medical Care.*	The authors assess the effects of a copayment change among enrollees of the Kaiser-Permanente Northwest division in the 1980 to 1990 time period. They used administrative data from the insurer on benefit design and medical and drug claims and estimated changes in drug utilization after the copayment change.	A $2 (66 percent) increase in copayment resulted in an 8 percent decrease in prescription use. The implied price elasticity of demand is **-0.12**.
Hillman et al. (1999). "Financial Incentives and Drug Spending in Managed Care." *Health Affairs.*	A large sample of members enrolled in nine different United HealthCare Corporation's insurance plans (N=134,937) is used to study the effect of higher copayments on drug utilization. The authors assess effects separately for physicians who are compensated under independent practice association (IPA) models and network-model HMOs. Analyses include plan fixed effects to control for potential selection that may bias results.	For individuals in IPA plans, a 50 percent increase in drug copayments led to a 12.3 percent decrease in drug spending (implied elasticity is **-0.25**). For individuals in network plans, a 50 percent increase in drug copayments led to only a 3.4 percent (statistically insignificant) reduction in drug spending (implied elasticity is **-0.07**).
Joyce et al. (2002). "Employer Drug Benefit Plans and Spending on Prescription Drugs." *JAMA.*	The authors study the effect of copayment changes on total drug spending, using 1997-99 data on non-elderly beneficiaries who worked at large firms with insurance benefits (N=420,786 beneficiaries). In the sample, only two of the 25 firms gave employees a choice of drug plans, which minimizes potential selection bias.	The price elasticity of drug expenditures was **-0.22** for single-tier plans and **-0.33** for two-tier plans.
*Goldman et al. (2004). "Pharmacy Benefits and the Use of Drugs by the Chronically Ill." *JAMA.*	The authors estimate how changes in cost sharing affect drug utilization (measured in drug days) of the most commonly used drug classes among the privately insured and chronically ill. They use 1997 to 2000 pharmacy claims data linked with health plan benefit designs from 30 employers (N=528,969 non-elderly beneficiaries).	For all 8 therapeutic classes analyzed, doubling copayments is associated with reductions in utilization. The largest decreases were for NSAIDs (elasticity estimate was **-0.45**) and antihistamines (elasticity was **-0.44**). Patients with at least one chronic illness were less responsive to price changes. Patients with arthritis, for example, had a price elasticity of demand for NSAIDs of **-0.27**.
*Landsman et al. (2005). "Impact of 3-Tier Pharmacy Benefit Design and Increased Consumer Cost-Sharing on Drug Utilization. *American Journal of Managed Care.*	The authors estimate price responsiveness of prescription demand for nine therapeutic classes using 1999 to 2001 data on three managed care populations whose pharmacy benefits changed from a 2-tier to a 3-tier design, compared with a managed care population that had no change in benefit design. Utilization was measured as the	The study found lower elasticities for drugs used in asymptomatic conditions (**-0.10 to -0.16** for statins, ACE inhibitors, CCBs, and ARBs) and higher elasticities for drugs used in symptomatic conditions (**-0.24 to -1.15** for triptans, SSRIs, Cox-e inhibitors, NSAIDs, and TCAs). The elasticity for NSAIDs was **-0.60**.

	average monthly number of prescriptions.	
Gibson, McLaughlin, & Smith. (2005). "A Copayment Increase for Prescription Drugs: The Long-Term and Short-Term Effects on Use and Expenditures." *Inquiry.*	The study exploits a natural experiment in which a large firm increases drug copayments. The authors use a panel dataset that provides information on medical and drug claims for the firm that changed copayments and a control firm that did not change copayments for the years 1995 to 1998 (N=263,000 employee quarters).	The overall elasticity of demand for drugs **is -0.04**. The own-price elasticity of demand for multisource brand-name drugs is **-0.27**, more elastic than that of single-source brand name drugs (elasticity is **-0.03**). The study does not find evidence that brand-name and generic drugs are substitutes.
Gaynor, Li, & Vogt. (2007). "Substitution, Spending Offsets, and Prescription Drug Benefit Design." *Forum for Health Economics & Policy.*	The authors use the 1997 to 2003 MarketScan panel dataset of insurance claims and benefit design (N=1.7 million person years) to assess the effects of changes in employer-provided drug benefits on drug spending. During this time, a number of employers reduced generosity of drug coverage. The model includes individual fixed effects approach to control for potential selection bias.	The short-run price elasticity of demand for drug spending with respect to price is **-0.6** and long-run elasticity is **-0.8**.
Shea et al. (2007). "Estimating the Effects of Prescription Drug Coverage for Medicare Beneficiaries." *Health Services Research.*	The authors use the 1999 Medicare Current Beneficiary Survey (N=5,270 beneficiaries) to identify the effect of insurance coverage on prescription utilization by Medicare beneficiaries. The authors use a multistage residual inclusion method using instrumental variables to control for selection bias.	Prescription drug insurance increased the number of prescriptions filled by 50 percent. The estimated price elasticity of demand for prescription drugs for Medicare beneficiaries is **-0.54.**
Chernew et al. (2008). "Effects of Increased Patient Cost Sharing on Socioeconomic Disparities in Health Care." *Journal of General Internal Medicine*.	This study explores whether the impact of increased drug copayments for diabetes and heart disease drugs differs between high- and low-income areas. The authors use MarketScan claims data which provide information on insurance coverage and claims for people covered by large employer plans (N=43,000 individuals with diabetes or heart disease).	The elasticity of demand on drug adherence ranges from **-0.03 to -0.05**. Those with lower income were more price-sensitive.
Gilman & Kautter. (2008). "Impact of Multitiered Copayments on the Use and Cost of Prescription Drugs Among Medicare Beneficiaries." *Health Services Research.*	This paper studies the impact of multi-tiered copayments on the cost and use of prescription drugs among Medicare beneficiaries. The authors use 2002 Marketscan data to link plan enrollment and benefits with medical and drug claims for 352,760 Medicare beneficiaries. They use cross-sectional variation in copayment structures among firms that offer employer-sponsored retiree health plans. To reduce potential selection bias, the authors ensure that each firm in their sample offers only one prescription drug plan, either a one-	Beneficiaries in three-tiered plans had lower drug utilization and higher OOP costs than individuals in lower-tiered plans. The price elasticity of demand for prescription drug expenditures is **-0.23.**

	tiered plan or a three-tiered plan.	
Chandra, Gruber, & McKnight. (2010). "Patient Cost-Sharing and Hospitalization Offsets in the Elderly. *American Economic Review*.	The authors exploit a policy change that raised cost sharing for patients covered by insurance plans for retired public employees in California. They use administrative data that provides information on medical utilization (N=70,912 continuously-enrolled individuals), and estimate DD models to identify the impact of increased copayments on drug utilization.	For PPO enrollees, the arc-elasticity of drug utilization (measured by number of prescriptions) with respect to patient cost is **-0.08**, and for HMO enrollees, the arc-elasticity is **-0.15.**
*Gatwood et al. (2014). "Price Elasticity and Medication Use: Cost-Sharing Across Multiple Clinical Conditions." *Journal of Managed Care & Specialty Pharmacy.*	The study sample consists of about 11.5 million privately insured enrollees aged 18 to 64 in the 2005-09 MarketScan claims database. The authors estimate negative binomial fixed effects models with patient cost sharing as the key independent variable and prescription fills as the outcome variable, separately for eight categories of drugs. Models include plan fixed effects, and thus focused on longitudinal changes in cost-sharing over time.	Elasticities range from **-0.02 to -0.16**, with the largest (in magnitude) price elasticity for smoking deterrents and the smallest for NSAIDs/opioids. Demand for antiplatelet agent was not responsive to price.
Yeung et al. (2016). "Price Elasticities of Pharmaceuticals in a Value-Based-Formulary Setting." NBER Working Paper.	The authors exploit a natural experiment that involved a large nonprofit insurance company transitioning its cost-based formulary to a value-based formulary, which tries to incentivize patients to use drugs that are likely to produce better value. This led to exogenous increases in cost-sharing for some drugs and decreases for others.	The overall price elasticity of demand for drugs is **-0.16**, but there is substantial variation across the formulary tiers, ranging from **-0.09 to -0.87**. Patients were more price-sensitive to drug placed in higher cost-sharing tiers.
Studies Based on the RAND Health Insurance Experiment		
Newhouse & the Insurance Experiment Group. (1993). *Free For All? Lessons from the Health Insurance Experiment.*	The Health Insurance Experiment randomly assign 5,800 non-elderly individuals to insurance plans with four different levels of coinsurance (ranging from 0 to 95 percent) and three different levels of maximum OOP expenditures.	Individuals in the free care plan spent nearly twice as much on prescription drugs as individuals in the 95 percent coinsurance plan ($82 and $46, respectively). However, the increase was attributable to a larger number of physician visits for individuals in the generous plan.[37] The overall elasticity estimate for prescription drugs is **-0.17**, similar to the elasticity of demand for health care in general.
Studies Based on Natural Experiments in non-US Settings		
O'Brien. (1989). "The Effect of Patient Charges on	The United Kingdom's National Health Service implemented copayments for	The price elasticity of demand for drugs was **-0.23** for the initial period

[37] The insurance plans in the RAND Health Insurance Experiment did not vary cost-sharing for prescription drugs independently of other medical services. Since prescription drugs may serve as substitutes or complements to other services, it is difficult to isolate the effect of drug prices on drug utilization using the RAND data.

the Utilization of Prescription Medicines." *Journal of Health Economics.*	prescription drugs in 1968. This study exploits the natural experiment to study the effect of OOP price increases on the number of prescriptions.	(1969-1977) and later rose to **-0.64** (1978-1986). The study also found a positive cross-price elasticity of 0.22 between prescription and OTC drugs.
Hughes & McGuire. (1995). "Patient Changes and the Utilization of NHS Prescription Medicines." *Health Economics.*	The authors exploit the 1968 implementation of copayments for prescription drugs in the United Kingdom's National Health Service. They use cointegration models to estimate price elasticities of demand for prescription drugs.	The price elasticity of demand for drugs is **-0.35.**
Contoyannis et al. (2005). "Estimating the Price Elasticity of Expenditure for Prescription Drugs in the Presence of Non-Linear Price Schedules: An Illustration from Quebec, Canada. *Health Economics*.	This study uses an exogenous change in cost-sharing within the Quebec public prescription drug insurance program to estimate price elasticity of expenditure for drugs using an instrumental variables approach. The instrument is based on the price an individual would face under the new policy if their consumption remained at the pre-policy level. The authors use administrative data on the Quebec program (N=120,000 elderly beneficiaries). Those without a prescription in the pre-policy period are not included in the analysis.	Expenditure elasticities range from **-0.12 to -0.16**.

*Studies that estimate elasticities for specific drug classes.

1-C. Additional Details about the MEPS Data

Construction of the MEPS Analytical Dataset

This section describes how I edited the original MEPS Prescribed Medicines files for the analysis in this paper. Step 1 describes how I merged the MEPS and CDC files. Steps 2 to 9 outline how I identified the opioid and non-opioid painkillers in MEPS. I could not simply use the Multum Lexicon codes provided by MEPS because the classification scheme changed over time (Hill, Roemer, & Stagnitti, 2014). I also could not use the NDCs because they were missing for 8 percent of the observations. I instead used the drug names provided by MEPS to identify opioids and non-opioid painkillers. Appendix Table 1- 2 displays a comprehensive list of each of the generic drug names in the opioid and non-opioid painkillers categories. Steps 10-15 explain

how I imputed missing information on opioids' MME, DEA schedule, etc. for the observations that were missing this information.

- **Step 1**: The original MEPS Prescribed Medicines files contained 5,652,749 observations for the years 1996 to 2015, where each observation represented the purchase or refill of a prescription medicine. Using the NDCs, I merged in additional information on MME, DEA schedule, extended vs immediate release, etc. for the opioid observations using the CDC Oral MME Equivalents file (https://www.cdc.gov/drugoverdose/data-files/CDC_Oral_Morphine_Milligram_Equivalents_Sept_2017.xlsx). The CDC file successfully matched with 89 percent of the opioid observations in the MEPS file (where opioid observations were defined as those in the Multum classes "Narcotic Analgesics" and "Narcotic Analgesic Combinations").
- **Step 2**: I browsed through the 486,003 observations that were classified as "Analgesics" by the Multum Lexicon codes and identified 510 observations that were misclassified as analgesics. These were primarily birth control pills, antibiotics, statins, vitamins, eyedrops, and antihistamines. I reclassified them in their correct categories. The sample now consisted of 485,493 analgesic observations.
- **Step 3**: Of the 485,493 analgesic observations, 3,661 were missing both NDCs and drug names. For these observations, I renamed the drug names to "Unknown Opioids" and "Unknown Non-Opioid Painkillers" according to their Multum codes.
- **Step 4**: There were 726 analgesic observations for which the drug names were missing but the NDCs were not. For these observations, I used the FDA's NDC database (https://www.accessdata.fda.gov/scripts/cder/ndc/) to look up the drug names.
- **Step 5**: For the remaining 481,106 analgesic observations, I streamlined the product names to correct misspellings, abbreviations, and other inconsistencies in the original MEPS drug names. For example, the drug "Acetaminophen" was spelled almost 70 different ways in the MEPS files ("ACEMINOPHEN", "ACETAMIN 120MG", "ACETAMINOPHEN DROP", "ACETAMI", etc). I created a variable called Product_Name that was spelled "Acetaminophen" for all such observations. I repeated this for all 481,106 analgesic observations and ended up with 663 distinct product names.
- **Step 6**: For each of the 485,493 analgesic observations, I created a variable to identify the generic drug names by looking up the drugs on the FDA's NDC database (https://www.accessdata.fda.gov/scripts/cder/ndc/). I ended up with 132 distinct generic drug names.
- **Step 7**: I browsed through all 5,652,749 observations and identified 1,071 observations that were actually analgesics based on their drug names but had been misclassified as non-analgesics by the MEPS Multum Lexicon codes. For example, in some cases, drugs like Aspirin and Vicodin were classified as muscle relaxants rather than analgesics. For these observations, I reclassified them as analgesics and streamlined their product names and generic drug names as described in Steps 5-6. I also browsed through the analgesic observations and reclassified treatment drugs for opioid use disorder (Buprenorphine,

Naloxone) as non-analgesics. I had now identified a total of 486,392 analgesic observations in the MEPS (485,493 correctly classified analgesics + 1,071 analgesics that had previously been misclassified – 172 opioid treatment drugs).

- **Step 8**: I used the generic drug names (a variable which I had manually created) to categorize all 5,652,749 observations into three categories: opioid painkillers (229,921 observations), non-opioid painkillers (256,471 observations), and other drugs (5,166,357 observations). I did not use the Multum Lexicon codes to distinguish opioid vs non-opioid painkillers because the Multum Lexicon codes were not consistent over time. For example, the drugs "Tramadol" and "Tramadol & Acetaminophen" were classified as "Miscellaneous Analgesics" from 1996 through 2011 but as "Narcotic Analgesics" from 2012 onwards.
- **Step 9**: I used the generic drug names (which had 132 distinct values) to create a binary variable that identified each individual drug. (For example, the variable "presc_tapen" was equal to 1 for all observations of Tapentadol prescriptions.)
- **Step 10**: Of the 229,921 opioid observations that I had identified in the MEPS, 89 percent had successfully matched with the CDC file (from Step 1). For the remaining 11 percent of opioid observations, I identified whether they were immediate release or extended release drugs through an imputation process. If an observation was missing immediate vs extended release information, I first used information provided in the MEPS drug name. (For example, I coded "MORPHINE IR" and "OXYCODONE 15MG IMM REL TABLETS" as immediate release formulations.)
    - For those that were still missing, I searched for drugs with the same name, form, and strength level in the CDC file. (For example, in the CDC file, all Fentanyl tablets were immediate release and all Fentanyl patches were extended release, so I identified missing Fentanyl tablets as immediate release and missing Fentanyl patches as extended release formulations.
    - For those that were still missing, I looked up the NDCs on the FDA website (https://www.fda.gov/Drugs/DrugSafety/InformationbyDrugClass/ucm251735.htm) and the Bioportal website (http://bioportal.bioontology.org/ontologies). Finally, for the 3,000 opioid observations that were missing both drug names and NDCs ("Unknown Opioids"), I identified them as "immediate release" because these were more prevalent in the data. I ended up with 185,748 "immediate release" opioids and 44,173 "extended release" opioids.
- **Step 11**: I identified DEA schedules of the 229,921 opioid observations. For 89 percent of the observations, this information was already available from the CDC file (from Step 1). For the remaining 11 percent of opioid observations, I imputed this information using steps similar to those described in Step 10. I ended up with 153,516 Schedule II opioid observations, 21,065 Schedule III, 53,904 Schedule IV, and 1,436 Schedule V.
- **Step 12**: I identified the drug form, strength per unit, and unit of measurement for the 229,921 opioid observations. If needed, I converted strength per unit from the given units to a consistent unit for all observations (MG for tablets, MG/ML for solutions, and MG/patch for patches). For 89 percent of the observations, this information was already

available from the CDC file (from Step 1). For the remaining 11 percent of opioid observations, I imputed this information. I first used the rxstreng and rxstrunt variables provided by MEPS to fill in missing information for strength per unit. (For example, if observations with an rxname of "Codeine" and rxstreng of "30 MG" did not match with the CDC file, I filled in "30" for the strength per unit and "MG" for the unit of measurement. This process allowed me to identify the drug form and strength per unit of 76 percent of the missing data.

  - For observations that had missing values for rxstreng and rxstrunt, I searched for drugs with the same name and form in the CDC file and used the modal values. (For example, the drug "Stadol" in its tablet form always had a strength per unit of 10 and unit of measurement of 10 MG/ML in the CDC file. So for Stadol observations with missing rxstreng values, I filled in "10" for the strength per unit and "MG/ML" for the unit of measurement.)
  - Finally, for the 3,000 opioid observations that were missing both drug names and NDCs ("Unknown Opioids"), 28 percent of them did have nonmissing information for rxstreng. For the remaining 72 percent, I identified the missing information using the modal values of all the opioid observations (i.e. "tablet" drug form, 5 for strength per unit, and MG for unit).
  - Through this process, I was also able to identify drug names for 26 percent of the 3,000 opioid observations that were missing both drug names and NDCs (for example, if the drug form was weekly patch and the category was narcotic analgesic, I knew the drug must be fentanyl). I now had only 2,210 opioid observations that were missing both drug names and NDCs ("Unknown Opioids").
  - I discovered that some of the "Unknown Opioids" were actually treatment drugs such as Buprenorphine or Naloxone. I reclassified these observations as non-analgesics. I now had 229,280 opioid observations.

- **Step 13**: For each of the opioid observations, I identified the active opioid ingredient (e.g. morphine, hydrocodone, fentanyl, tramadol, etc) using the generic drug names. For the 2,210 opioid observations that were missing both drug names and NDCs ("Unknown Opioids"), I listed the active opioid ingredient as "Unknown."
- **Step 14**: For each of the opioid observations, I identified the MME conversion factor. I had obtained this data from the CDC file for 89 percent of the opioid observations (see Step 1). For the remaining 11 percent, I used the active opioid ingredients and obtained this information from the CMS website (https://www.cms.gov/Medicare/Prescription-Drug-Coverage/PrescriptionDrugCovContra/Downloads/Opioid-Morphine-EQ-Conversion-Factors-Aug-2017.pdf). For the 2,210 opioid observations that were missing both drug names and NDCs ("Unknown Opioids"), I identified the MME conversion factor as the modal MME (1).
- **Step 15**: MEPS provided the total amount spent on each prescription, as well as the breakdown by source of payment (amount paid by self, private insurance, other private sources, workers' compensation, Medicare, VA, Champus, Tricare, other federal sources,

Medicaid, other state/local sources, other public sources, and other sources). These variables were all nonmissing in the original data because they had already been imputed by MEPS for missing cases. I used these variables to calculate amount paid by all public sources (sum of workers' compensation, Medicare, VA, Champus, Tricare, other federal sources, Medicaid, other state/local sources, and other public sources) and amount paid by all private sources and self (sum of private insurance, other private sources, self, and other).

- **Step 16**: I imputed quantities and days supplied for the opioid and non-opioid painkillers, ensuring that the units matched the unit of measurement from Step 12. Of the 485,751 painkiller observations, the MEPS quantity variable (rxquanty) was nonmissing for 99.9 percent of observations. For those 561 observations that were missing quantities, I imputed the quantity by using the modal quantity of the same NDC in other cases.
    - The days supplied variable was only provided for the years 2010 onwards. Of the 485,751 painkiller observations, I had days supplied information for only 23 percent. Before any imputations, the mean (median) days supplied per prescription was 18 (16) for opioids and 31 (30) for non-opioid painkillers.
    - For observations that were missing days supplied, I imputed using the modal quantity per day supplied of the same NDC in cases for which I did have days supplied. (For example, for NDC 00054024425 (Codeine 30 mg tablets), the mode number of tablets patients were prescribed per day in the post-2010 period was 6. Therefore, for NDC 00054024425 in the pre-2010 period, I coded the days supplied variable as the quantity of tablets in the prescription divided by 6.) After this imputation, I had days supplied information for 61 percent of the painkiller observations.
    - For cases where the NDC was not observed again in the post-2010 period, I imputed using the modal quantity per day supplied for observations that had the same product name, drug form, and strength level. After this imputation, I had days supplied information for 90 percent of the painkiller observations.
    - For cases where days supplied was still missing, I imputed using the modal quantity per day supplied for observations that had the same generic drug name, drug form, and strength level. After this imputation, I had days supplied information for 95 percent of the painkiller observations.
    - For cases where days supplied was still missing, I imputed using the modal quantity per day supplied for observations that had the same generic drug name and drug form. After this imputation, I had days supplied information for 100 percent of the painkiller observations.
    - After all the imputations, the mean (median) days supplied per prescription was 16.5 (10) for opioids and 28.2 (30) for non-opioid painkillers.
- **Step 17**: For each opioid observation, I multiplied the Quantity variable (from MEPS) with the Strength Per Unit variable and the MME conversion factor (from the CDC file) to obtain the total MMEs in each prescription. Prior to calculating the product, I ensured that all three variables were measured in the same units.

    - Based on the total MME per day supplied, I identified high-dose opioid prescriptions as those that had more than 90 MMEs per day supplied and low-dose prescriptions as those that had 90 or fewer MMEs per day supplied.
- **Step 18**: Before collapsing the data, I created additional spending variables that described the amount spent (in that transaction) by each payment source on each of the generic drugs, extended release opioids, immediate release opioids, high-dose opioids, and low-dose opioids.
- **Step 19**: I collapsed the data at the prescription level to obtain a person-year level dataset that provided the number of prescriptions and amount of money spent for each drug type. I then calculated the amount spent per prescription for each drug type.
- **Step 20**: I collapsed the data at the days supplied level to obtain a person-year level dataset that provided the number of days supplied and amount of money spent for each drug type. I then calculated the amount spent per day supplied for each drug type.

MEPS Limitations

This subsection describes how I handle limitations of the MEPS data in my analysis. The text in italics comes from the MEPS codebook and methodology report (Agency for Healthcare Research and Quality, 2017; Hill et al., 2014), and subsequent paragraphs explain the extent to which the methodological issue does or does not threaten the validity of my results.

1. *"Users should carefully review the data when conducting trend analyses or pooling years or panels because Multum's therapeutic classification has changed across the years of the MEPS…Analysts should use caution when using the Cerner Multum therapeutic class variables for analysis and should always check for accuracy."*

I do not use the Multum therapeutic variable to classify drugs, since the Multum codes change over the time period of my analysis. Rather, I carefully identify opioid and non-opioid painkillers by using the original drug names provided by MEPS.

2. *"…beginning with the 2007 data, the rules MEPS uses to identify outlier prices for prescription medications became much less stringent than in prior years. Starting with the 2007 Prescribed Medicines file, there was: less editing of prices and quantities reported by pharmacies, more variation in prices for generics, lower mean prices for generics, higher mean prices for brand name drugs, greater differences in prices between generic and brand name drugs, and a somewhat lower proportion of spending on drugs by families, as opposed to third-party payers."*

The DD model estimates the treatment effect as the difference between the treatment group (individuals aged 65-74) and the control group (individuals aged 55-64) after 2006, relative to the difference between the two groups before 2006. Presumably, the 2007 MEPS methodological changes were applied to all respondents without age-based discrimination. As long as the rules were not applied differentially to my treatment group and control group after 2007, the DD results should not be affected.

3. *"Starting with the 2008 Prescribed Medicines file, improvements in the data editing changed the distribution of payments by source: (1) more spending on Medicare beneficiaries is by private insurance, rather than Medicare, and (2) less out-of-pocket payments and more Medicaid payments among Medicaid enrollees."*

My interest is in OOP drug prices, so the shift from Medicare to private insurance among Medicare beneficiaries is not relevant for my analysis. I estimate a sensitivity analysis in which I omit Medicaid enrollees from analysis, and I find that the substantive results are similar (results available on request).

4. *"Starting with the 2009 data, additional improvements increased public program amounts and reduced out-of-pocket payments and, for Medicare beneficiaries with both Part D and Medicaid, decreased Medicare payments and increased Medicaid and other state and local government payments."*

Regarding the reductions in OOP payments, so long as the methodology for calculating OOP payments did not change differentially for the treatment and control groups in 2009, my DD model should still capture the causal effect of Part D. I am primarily interested in the OOP gap between the treatment and control groups, not the raw levels of OOP payments. Regarding the second issue (decreased Medicare payments and increased Medicaid payments), my interest is in OOP prices, so the shift from Medicare to Medicaid among dual eligibles is not relevant for my analysis.

Other methodological changes were made beginning with the 2010 data, such as improvements to account for price discounts in the Part D donut hole and improvements in the price imputation methodology. However, since my period of analysis covers only through 2009, these later changes do not affect my results.

1-D. Background on Medicare Part D

Established in 1966, Medicare provides health insurance for individuals over age 65. For the first 40 years of its existence, however, the Medicare program did not provide prescription drug coverage, with the exception of drugs administered in institutional settings such as hospitals and physicians' offices. Before 2006, the elderly had limited access to drug coverage: some low-income "dual-eligible" Medicare beneficiaries received coverage through Medicaid or state-sponsored drug programs; others received coverage through their employers or purchased coverage themselves through Medigap policies offered by private firms. However, these plans were often expensive and had caps on drug spending; one study found that before 2006, nearly one-third of elderly enrollees with drug coverage faced annual caps of $500 or less (Gold, 2001). Because of all these challenges, nearly one-third of Medicare beneficiaries lacked drug coverage before 2006 (Kaestner & Khan, 2012). Without insurance, these adults faced considerable cost barriers in accessing drugs and were more likely to engage in cost-related nonadherence (Duggan, Healy, & Scott Morton, 2008).

Motivated by the high proportion of elderly adults without drug coverage, high out-of-pocket spending burdens for the uninsured, and growing clinical importance of drugs in preventing and treating disease, the federal government established a prescription drug benefit for the elderly as part of the Medicare Modernization Act of 2003 (MMA). As of January 1, 2006, Medicare beneficiaries gained access to drug coverage through Medicare Part D

(henceforth referred to as "Part D"). Insurance was delivered through private Part D plans and subsidized by the federal government. The MMA also provided a means-tested subsidy to help cover premiums and cost sharing for low-income individuals with limited assets.

How Part D Works

The enactment of Part D affected Medicare beneficiaries differently depending on their prior drug coverage (Levy & Weir, 2009):

1. Those who already had creditable drug coverage (e.g. through their current or former employers) were instructed to keep that coverage, and employers received subsidies from the government to continue offering it. (This was intended to reduce the likelihood that Part D would crowd out existing sources of drug coverage.)
2. Those on Medicaid (dual eligibles) were automatically enrolled in Part D and the subsidy.
3. Eighty-six percent of those on Medicare Advantage plans already had drug coverage before Part D. After 2006, nearly all Medicare Advantage plans included Part D plans as part of their benefit.
4. Those without coverage or with privately purchased drug coverage (including Medigap plans) could decide whether to enroll in Part D and whether to apply for the subsidy.

Part D beneficiaries could choose from three types of drug plans: 1) stand-alone plans that offered only drug coverage, 2) Medicare Advantage plans that provided all Medicare benefits including prescription drugs, or 3) creditable employer-sponsored coverage (for which the government would subsidize the employer). Enrollment in Part D plans was voluntary, but

recipients were subject to a financial penalty for each month that they delay enrollment after reaching the eligible age (to lessen adverse selection).

For a typical Part D plan in 2006, the enrollee was responsible for paying 100 percent of their drug spending until reaching a $250 annual deductible. For the next $2,250 of spending, the plan covered 75 percent and the enrollee paid the remainder out of pocket. For the next $3,600 of spending, the plan paid 0 percent and the enrollee paid 100 percent (this part was known as the "coverage gap" or "doughnut hole"). After spending reached $5,100, the plan paid 95 percent and the enrollee paid only 5 percent out of pocket (Engelhardt & Gruber, 2011). Insurers had substantial flexibility in plan design, so long as the plan was actuarially equivalent to the one described above and covered certain therapeutic classes of drugs.

Relevance of Part D for Researchers

The introduction of Part D represented the most significant expansion to Medicare since the program's inception. Appendix Figure 1- 3 shows that the prescription drug coverage rate for the elderly jumped from 74 percent before 2006 up to 92 percent in the years following Part D. Coverage for a control group of near-elderly individuals, on the other hand, increased only marginally from 81 percent to 84 percent over the same time period. Part D currently serves 41 million Medicare beneficiaries and spends $94 billion ($2,300 per beneficiary) each year (Kaiser Family Foundation, 2016). The policy has had large-scale impacts on prescription drug utilization, out-of-pocket spending, drug prices, and inpatient hospitalizations among elderly individuals.

The implementation of Part D is of particular interest to researchers because it generated substantial variation in drug coverage rates across age groups and over time. Those above age 65 received a positive shock in their out-of-pocket price of prescription drugs after 2006, whereas

those below 65 did not. Appendix Figure 1- 4 shows that after 2006, the share of elderly individuals' prescription spending attributable to Medicare increased substantially from 9 percent before 2006 to 49 percent after the implementation of Part D; meanwhile, the share of total spending spent out of pocket fell from 49 percent before Part D to 25 percent after. Spending shares for the control group of near-elderly individuals, on the other hand, remained largely constant before and after 2006. This suggests that Part D led to a large change in out-of-pocket drug spending for Medicare eligibles.

Prior research has exploited the implementation of Part D as a natural experiment for understanding the causal effects of prescription drugs on various health, financial, and social outcomes. Although Part D is an older policy, it continues to be used as a setting for studying prescription drug coverage even in recent studies (Bradford & Bradford, 2016; Buchmueller & Carey, 2018; Carey, 2017; Dunn & Shapiro, 2019; Huh & Reif, 2017; Kaplan & Zhang, 2017; Powell et al., 2017).

1-E. Additional MEPS Analysis

Appendix Figure 1- 5 displays trends in OOP prices of prescription painkillers over time for the outcomes not presented in Figure 1-4 in the main paper: price per MME and price per day supplied of high dose opioids, low dose opioids, extended release opioids and immediate release opioids. For the majority of outcomes, OOP prices appeared to follow similar trends for the treatment and control groups before 2006 and declined substantially for the treatment group after 2006. Appendix Figure 1- 6 displays similar trends for the utilization outcomes (comparable to Figure 1-5 in the main paper). Although levels of utilization are always higher for elderly individuals, the trends are largely similar for the treatment and control groups before 2006, followed with a large uptick in utilization for the treatment group after 2006.

In Appendix Table 1- 4, I use my baseline DD model to model the effect of Part D on utilization of all prescription drugs (not just painkillers). I find that Part D led to an increase in 2.95 prescriptions utilized per year ($p<0.01$), which represents an 11 percent increase over pre-2006 levels. The policy also reduced OOP prices by $7.61 per prescription, which represents a 24 percent decline from pre-2006. This implies a price elasticity of demand of -0.45, which aligns with findings from previous studies (Duggan & Scott Morton, 2010; Ketcham & Simon, 2008; Liu et al., 2011; Yin et al., 2008).

Appendix Table 1- 5 displays regression results for the impact of Part D on prescription opioid utilization by drug (to be compared with Table 1-4 in the main paper). The increased opioids utilization can be traced to large increases in hydrocodone (2.94 increase in days supplied or 134 percent increase from pre-2006) and morphine (1.00 increase in days supplied of 417 percent increase from pre-2006).

In Appendix Table 1- 6 through Appendix Table 1- 12, I present results from numerous parallel trends tests, falsification tests, and sensitivity analyses that provide confidence in the causal interpretation of my results. I discuss these results in detail in the main paper.

Appendix Table 1- 13 shows results from a specification in which the outcome variable is measured as an indicator for whether the respondent made any purchase of the prescription that year. The estimated treatment effects are close to zero and not statistically significant, suggesting that there was no impact of Part D on the extensive margin of painkiller utilization. This may be because painkiller utilization was already relatively among elderly individuals even before 2006. Thirty-five percent of elderly individuals used prescription painkillers, even before the introduction of Part D.

In Appendix Table 1- 14, I use my DD model to assess the effects of Part D on the number of prescriptions individuals receive as "free" samples. Providers or manufacturers may offer free samples as a way to market their drugs, and so if I were to find increases in the number of opioids offered as free samples, it may raise concerns about the possibility of non-price mechanisms influencing the purchase of painkillers after Part D. However, I find that there was no significant impact of Part D on the number of free samples of opioids. Moreover, while there was an impact for non-opioid painkillers, it was in the opposite direction as expected. Part D led to a 25 percent decline in the number of free samples of non-opioid painkillers, suggesting that advertising through this avenue actually fell.

1-F. Additional Nielsen Analysis

Before 2006, the average price per day supplied of an OTC painkiller was $0.37. Appendix Table 1- 15 shows that there was no detectable effect of Part D on the prices of OTC painkillers for older households relative to younger households. The DD coefficient is close to zero and statistically insignificant.

Appendix Table 1- 16 displays results from an event study specification that assesses differential trends in OTC utilization between the treatment and control group in each year, relative to the base year 2005. Older households purchase more painkillers than younger households. In the years 2004 and 2006, the gap between older and younger households increased, whereas during 2007-09, this gap shrunk substantially.

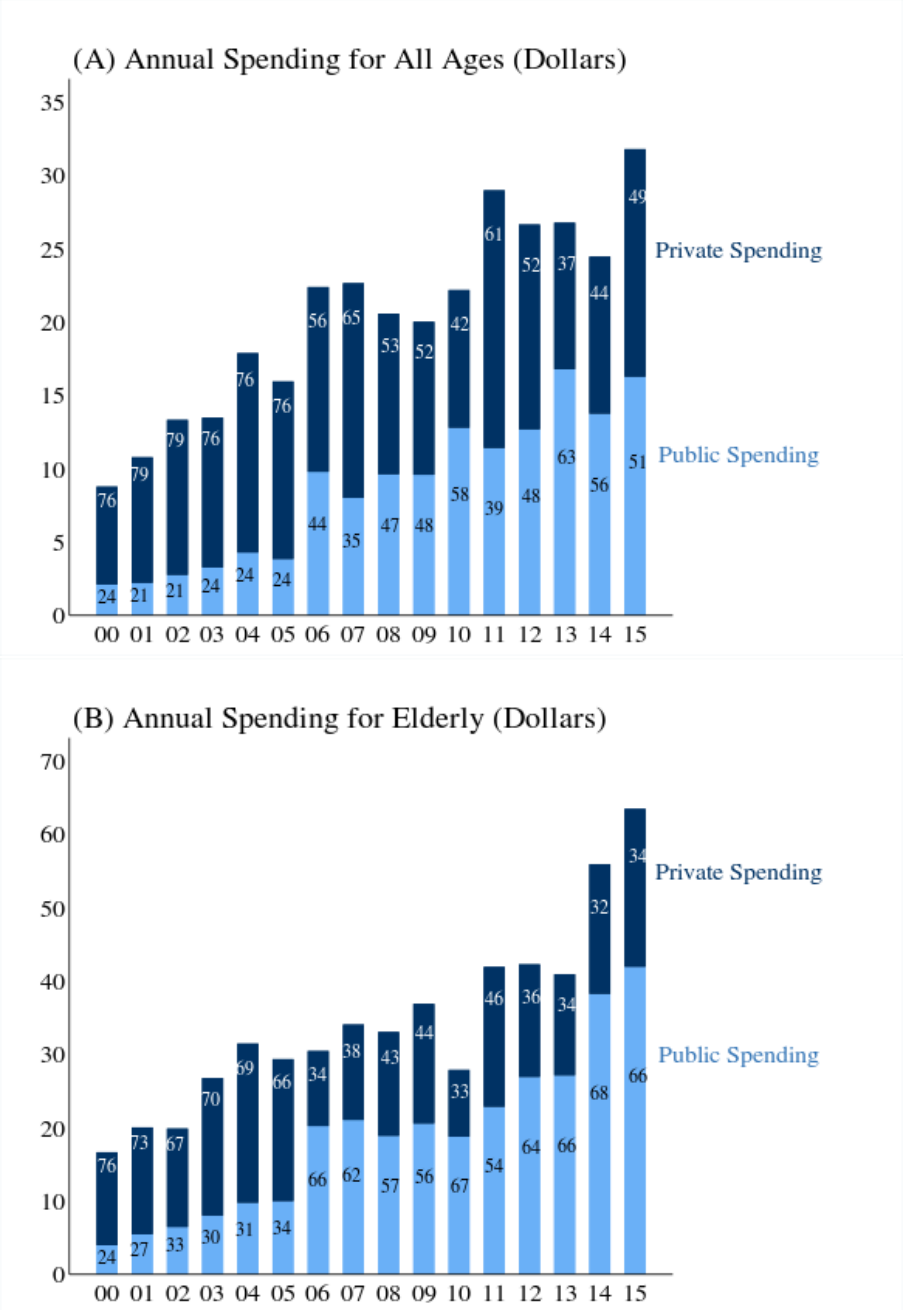
I expose the baseline DD model to a number of sensitivity analyses, and results are displayed in Appendix Table 1- 17. The baseline DD model presented in the main paper yields a treatment effect of -3.27 ($p<0.01$). Column 1 displays results from a specification that omits demographic control variables from the right hand side; in this specification, the treatment effect

is -3.46 ($p<0.01$). Column 2 shows that if Nielsen survey weights are omitted, the treatment effect is -3.46 ($p<0.01$). Both these results are remarkably similar to that presented in the original baseline model. However, when I omit household fixed effects from the right hand side, the DD coefficient is 0.18 and imprecisely measured. This suggests that the results are sensitive to the inclusion of household fixed effects. In Column 4, I show that Part D led to a 0.01 percentage point or 1.3 percent decline in the probability of purchasing any OTC painkillers in a given year.

Finally, I explore heterogeneous effects of the policy by income. Appendix Table 1- 18 shows that the decline in OTC painkillers was concentrated among high-income households with income greater than 400 percent of the poverty level and middle-income households with incomes between 125 and 400 percent of the poverty level. As expected there was no detectable effect of Part D on OTC painkiller utilization of low-income households because these individuals were more likely to have drug coverage through Medicaid even before Part D.

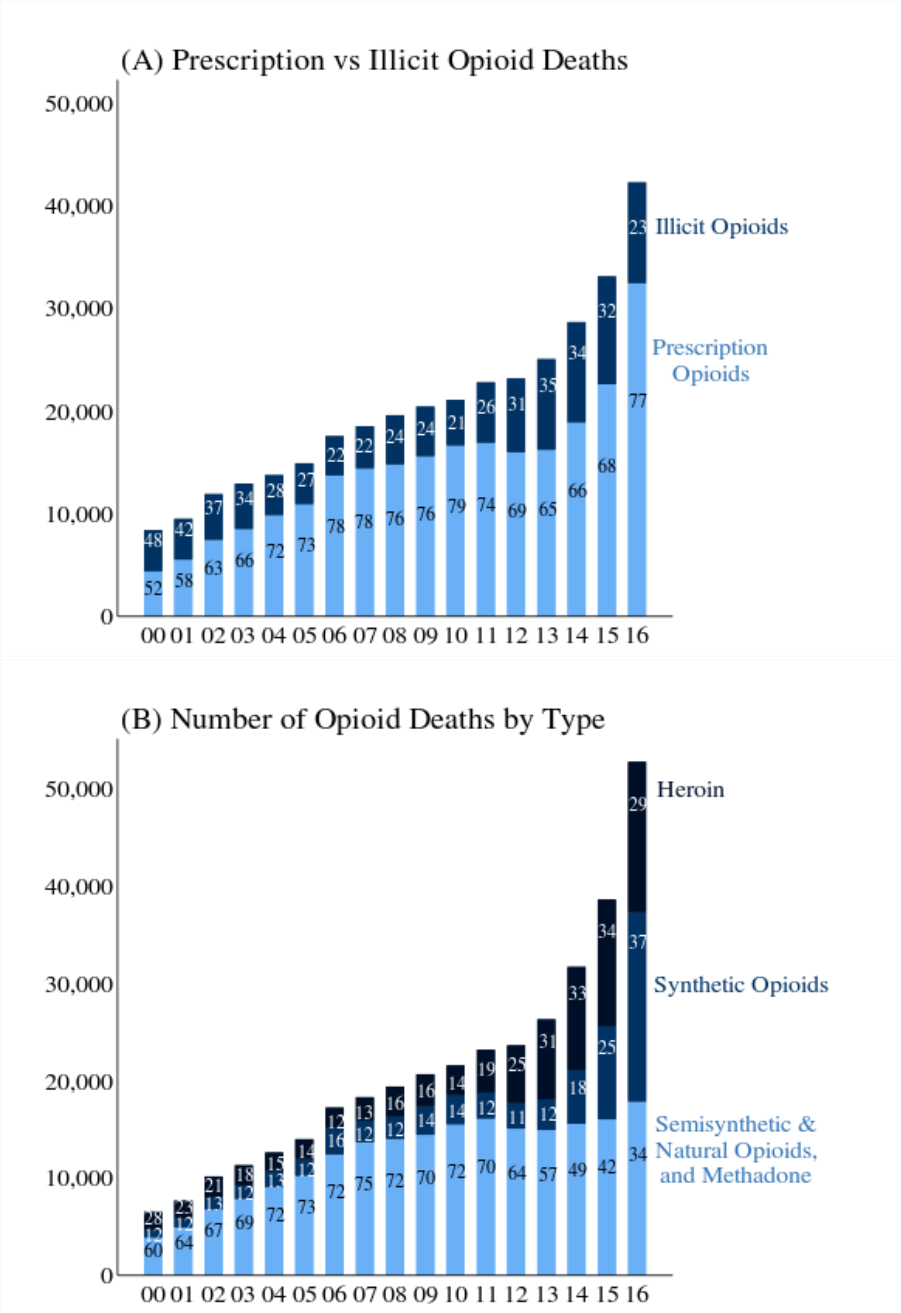
1-G. Appendix 1 Figures

Appendix Figure 1- 1. Prescription Opioid Spending per Person



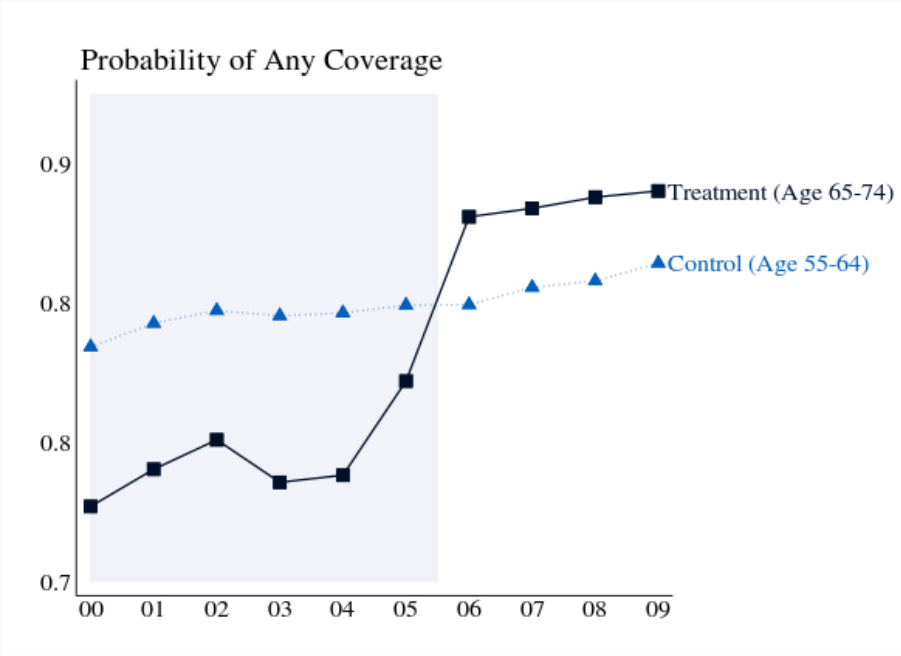
Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2015. Panel A includes all respondents (N=549,801), and Panel B includes respondents over age 65 (N=60,798). Figures display the mean number of painkiller prescriptions per person, adjusted by MEPS survey weights.

Appendix Figure 1- 2. Opioid Overdose Deaths by Type



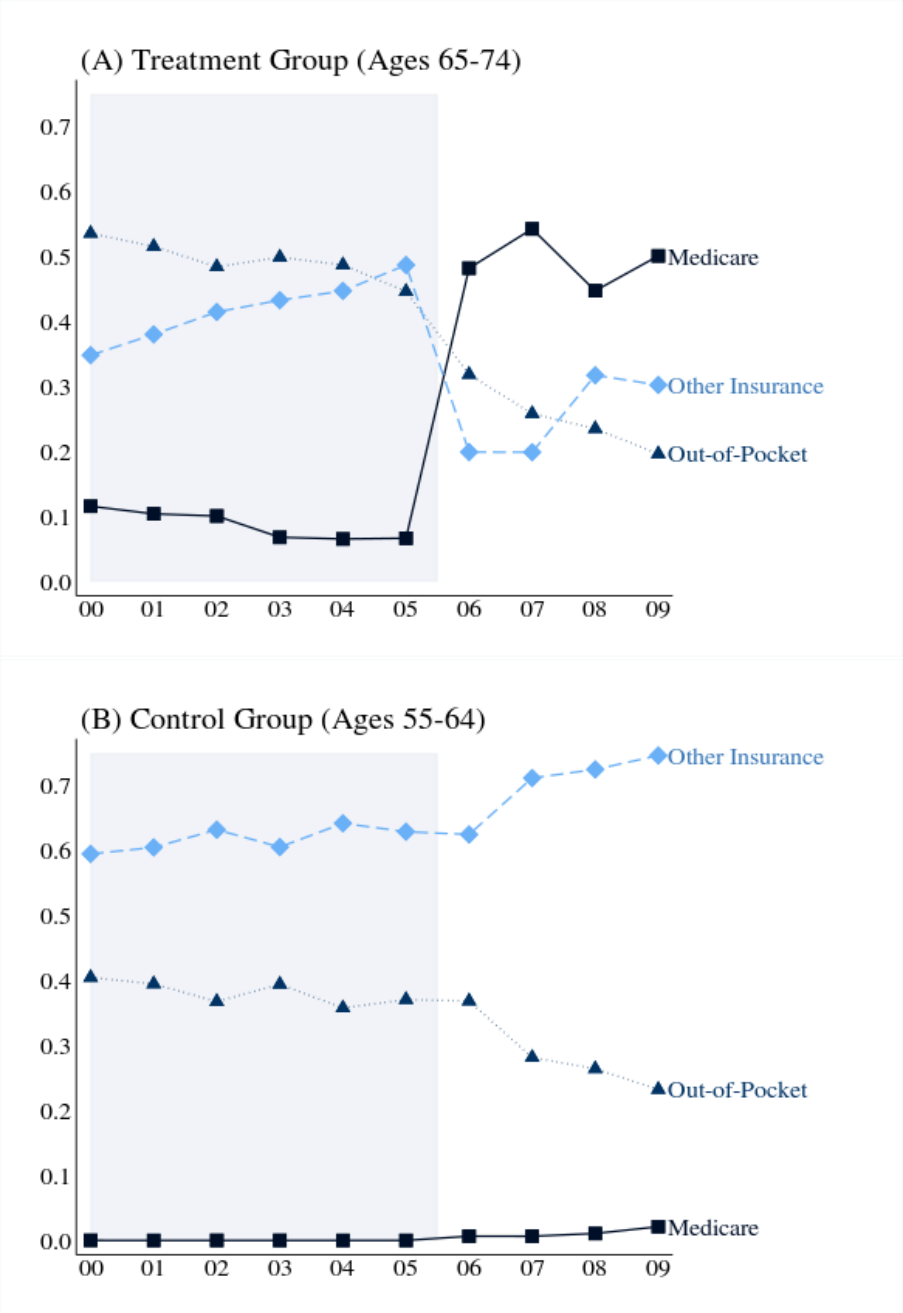
Source: Author's calculations based on data from the Henry J. Kaiser Family Foundation. Figures display the number of opioid overdose deaths in the United States by category. The numbers inside each bar indicate the percent of total opioid overdose deaths attributable to that category.

Appendix Figure 1- 3. Impact of Part D on Prescription Drug Insurance Rates



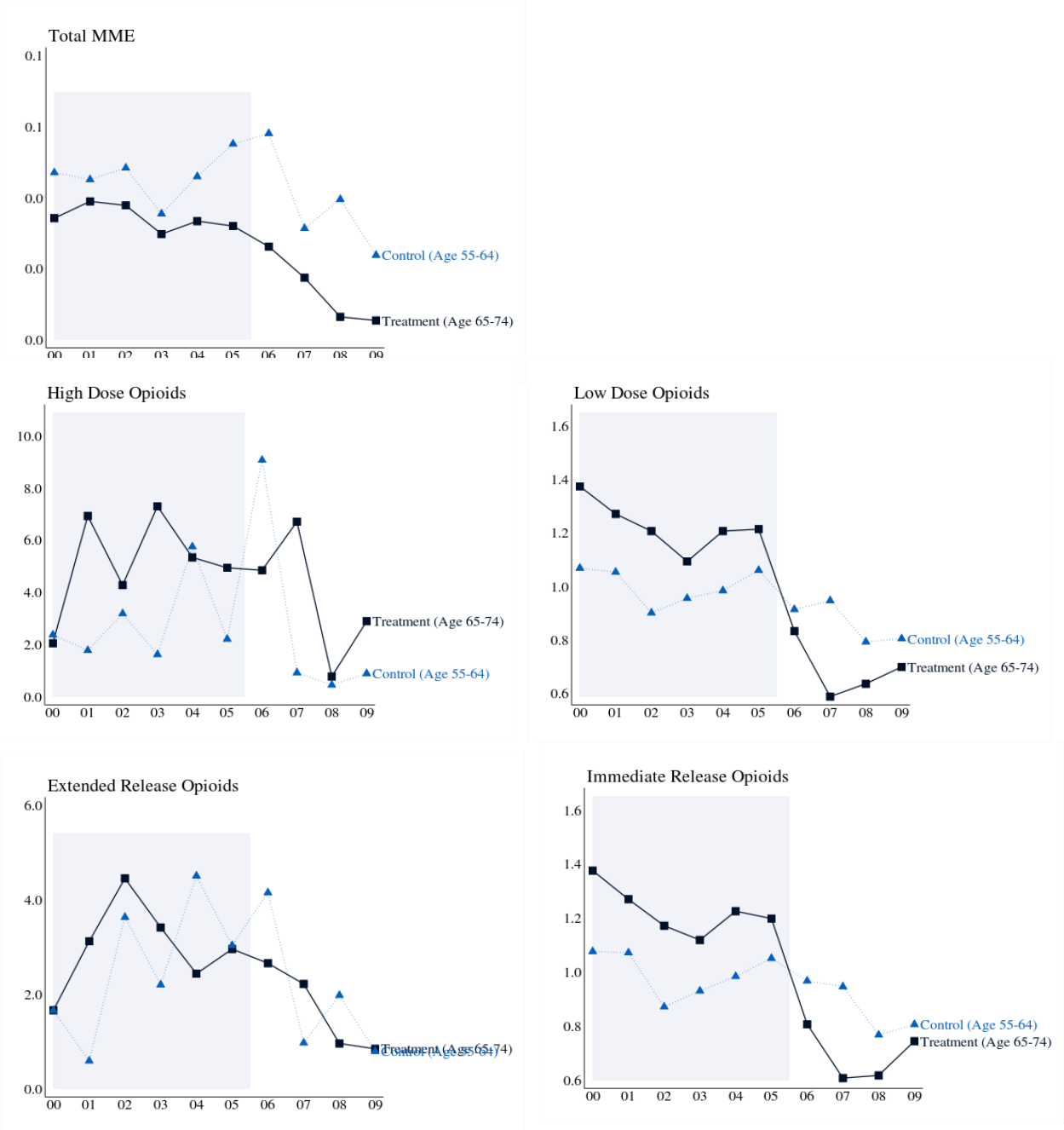
Source: Author's calculations based on Medical Expenditure Panel Survey. Sample is restricted to adults aged 55 to 74 (N=50,579). Figure displays probability of having any prescription drug coverage at any point during the year, adjusted by MEPS survey weights. Individuals are defined as having prescription drug coverage if at least one of the following is true: 1) they have a private source of insurance coverage, 2) they reported positive third party payments for prescriptions purchased during the year, or 3) they have a Medicare Part D plan.

Appendix Figure 1- 4. Proportion of Total Prescription Drug Spending by Source



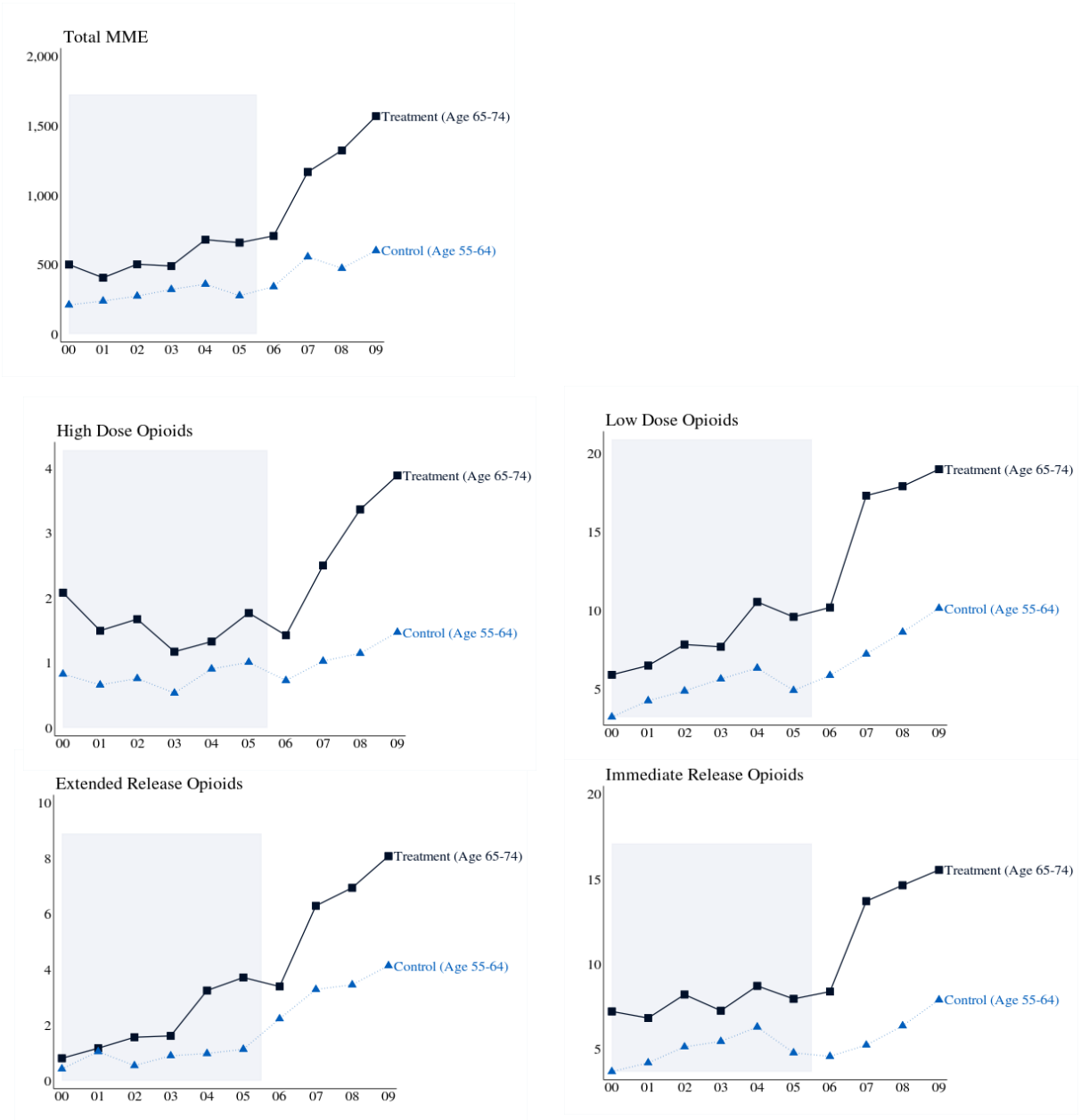
Source: Author's calculations based on Medical Expenditure Panel Survey. Sample is restricted to adults aged 55 to 74 (N=50,579). Figures display percentage of total prescription drug spending paid by each source, adjusted by MEPS survey weights.

Appendix Figure 1- 5. Out-of-Pocket Prices of Prescription Painkillers over Time



Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Figures display the mean OOP spending per day supply of each NDC, weighted by 2003 utilization of the NDC. Prices are adjusted to 2009 dollars using the Bureau of Labor Statistics' Pharmaceutical Producer Price Index

Appendix Figure 1- 6. Utilization of Prescription Painkillers over Time



Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74 (N=50,579). Figures display the mean number of days supplied per person, adjusted by MEPS survey weights.

1-H. Appendix 1 Tables

Appendix Table 1- 2. Classification of Prescription Painkillers in MEPS

Drug	Common Brands
Opioids – Prescription pain relief drugs whose distribution is controlled by the DEA because they have potential for abuse and may lead to psychological or physical dependence	
Butorphanol	Stadol
Codeine	
Codeine & Acetaminophen	
Dihydrocodeine & Acetaminophen	
Dihydrocodeine & Aspirin	
Fentanyl	Durageic, Actiq
Hydrocodone	Hysingla
Hydrocodone & Acetaminophen	Lortab, Lorcet, Vicodin, Norco
Hydrocodone & Aspirin	Damason
Hydrocodone & Ibuprofen	Vicoprofen
Hydromorphone	Dilaudid
Levorphanol	Levo Dromoran
Meperidine	Demerol
Meperidine & Promethazine	Meprozine
Morphine	MS Contin, Kadian, Avinza
Nalbuphine	Nubain
Opium	
Oxycodone	Oxycontin, Roxicodone
Oxycodone & Acetaminophen	Percocet, Endocet, Roxicet
Oxycodone & Aspirin	Endodan, Percodan
Oxycodone & Ibuprofen	Combunox
Oxymorphone	Opana
Pentazocine & Acetaminophen	Talacen
Propoxyphene	Darvon
Propoxyphene & Acetaminophen	Darvocet, Propacet
Propoxyphene & Aspirin	
Tapentadol	Nucynta
Tramadol	Ryzolt
Tramadol & Acetaminophen	Ultracet
Unknown Opioids	
Non-opioid painkillers – Pain relief drugs that are not controlled by the DEA but require a physician's prescription	
Acetaminophen & Acetaminophen Combinations	Fioricet, Mapap, Midrin, Tylenol
Almotriptan	Axert
Aspirin & ASA Combinations	Aspirin, Ecotrin, Fiorinal
Bromfenac	Duract
Celecoxib	Celebrex
Choline Magnesium Trisalicylate	Trilisate
Diclofenac	Arthrotec, Cataflam, Voltaren
Diflunisal	Dolobid
Dihydroergotamine mesylate	Migranal
Eletriptan	Relpax
Ergotamine	Cafergot
Etodolac	Lodine

Fenoprofen	
Flurbiprofen	Ansaid
Frovatriptan	Frova
Ibuprofen	Advil, Motrin
Indomethacin	Indocin
Ketoprofen	Oruvail
Ketorolac	Toradol
Magnesium salicylate	
Meclofenamate	
Mefenamic acid	Ponstel
Meloxicam	Mobic
Methylprednisolone	
Methysergide maleate	Sansert
Nabumetone	Relafen
Naproxen	Naprelan, Anaprox, Aleve
Naratriptan	Amerge
Oxaprozin	Daypro
Piroxicam	Feldene
Prednisone	
Rizatriptan	Maxalt
Rofecoxib	Vioxx
Salsalate	
Sulindac	Clinoril
Sumatriptan	Imitrex
Tolmetin	
Valdecoxib	Bextra
Zolmitriptan	Zomig
Unknown Non-Opioid Painkillers	

Source: Author's classification of drugs in Medical Expenditure Panel Survey Prescribed Medicines files.

Appendix Table 1- 3. Composition of the 2003 Basket of Pain Relief Drugs

Generic Drug Name	Sample NDCs	Proportion of All Painkillers	
		Treatment Group	Control Group
Opioids			
Hydrocodone & Acetaminophen	00406035705, 52544063401	0.031	0.051
Methadone	00406345434, 00054457025	0.025	0.007
Oxycodone	59011010010, 58177004104	0.021	0.015
Propoxyphene & Acetaminophen	00378015505, 00603546628	0.014	0.016
Oxycodone & Acetaminophen	00054465029, 00406053201	0.013	0.011
Tramadol	00045065960, 00378415105	0.010	0.005
Fentanyl	50458003405, 50458003505	0.008	0.001
Codeine & Acetaminophen	00045051360, 63304056201	0.007	0.008
Hydrocodone & Ibuprofen	00093516101	0.001	0.001
Codeine	00054415625	0.001	0.001
Morphine	60951065270	0.001	0.007
Hydromorphone	00406324301	0.001	0.001
Meperidine & Promethazine	00603442421, 58177002704	0.001	0.001
Propoxyphene	00603545921	0.001	0.001
Tramadol & Acetaminophen	00045065060	0.001	0.020
Non-Opioid Painkillers			
Celecoxib	00025152031, 00025152051	0.364	0.374
Aspirin & ASA Combos	00182044810, 15127022894	0.303	0.183
Diclofenac	00781178901, 00591033801	0.055	0.053
Meloxicam	00597002901, 00597003001	0.040	0.037
Naproxen	00093014901, 67253062210	0.028	0.070
Ibuprofen	00009738701, 49884077705	0.026	0.058
Acetaminophen & Combos	00603026321, 00143111501	0.010	0.018
Nabumetone	00093101501, 00029485120	0.010	0.029
Indomethacin	00172403060, 00378014301	0.013	0.001
Diflunisal	00093075506	0.004	0.001
Etodolac	51672401801, 00093112201	0.004	0.002
Oxaprozin	00185014101, 49884072301	0.004	0.012
Piroxicam	00093075701, 00378202001	0.003	0.006
Sulindac	00378053101, 00591566001	0.003	0.005
Flurbiprofen	00378009301, 00093071101	0.002	0.006
Ketoprofen	00378575001	0.001	0.001
Ketorolac	00378113401, 58177030104	0.001	0.001

Source: Author's calculations based on Medical Expenditure Panel Survey 2003. This table excludes drugs that were removed from the market before 2006 (i.e. Vioxx, Bextra, etc).

Appendix Table 1- 4. Impact of Part D on Utilization of All Prescription Drugs

	Utilization (Prescriptions)			Price (OOP Price per Prescription)			Elasticity
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)	Pre-2006 Mean (4)	DD Coefficient (5)	Percent Change (6)	(7)
All Drugs	26.27	2.95*** (0.82)	11.2%	31.52	-7.61*** (0.92)	24.1%	-0.45
N		50,579			50,579		

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Columns 1 and 4 display the pre-2006 mean for the treatment group. Columns 2 and 5 display the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. Columns 3 and 6 displays percent change from pre-2006 mean.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table 1- 5. Regression Results for Impact of Part D on Prescription Opioid Utilization (Days Supplied) by Drug

	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)
Hydrocodone	2.20	2.94*** (0.67)	133.6%
Propoxyphene	2.64	0.67 (0.51)	-
Oxycodone	1.44	0.42 (0.58)	-
Tramadol	1.60	0.50 (0.52)	-
Codeine	0.56	0.03 (0.16)	-
Morphine	0.24	1.00*** (0.32)	416.7%
Fentanyl	0.34	0.32 (0.35)	-
Methadone	0.55	-0.32 (0.30)	-
Other Opioids	0.32	-0.24* (0.13)	-75.0%
N		50,579	

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, column 3 displays percent change from pre-2006 mean.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 6. Event Study Results for Impact of Part D on Prescription Painkillers

Panel A: Utilization (Days Supplied)

	Total MME (1)	High Dose Opioids (2)	Low Dose Opioids (3)	Extended Release Opioids (4)	Immediate Release Opioids (5)
Year 2000 X Treatment	-74.74	0.54	-1.93	-2.16**	0.46
	(153.40)	(0.71)	(1.60)	(0.87)	(1.68)
Year 2001 X Treatment	-195.01	0.13	-2.36*	-2.42***	-0.44
	(139.42)	(0.67)	(1.37)	(0.87)	(1.36)
Year 2002 X Treatment	-142.99	0.18	-1.70	-1.51*	-0.09
	(189.07)	(0.70)	(1.56)	(0.91)	(1.52)
Year 2003 X Treatment	-221.74	-0.14	-2.79*	-1.93**	-1.46
	(147.02)	(0.58)	(1.64)	(0.93)	(1.56)
Year 2004 X Treatment	-46.92	-0.30	-0.35	-0.27	-0.63
	(123.19)	(0.53)	(1.50)	(0.87)	(1.43)
Year 2006 X Treatment	-54.51	-0.20	-0.65	-1.60	0.38
	(219.06)	(0.47)	(1.57)	(1.01)	(1.39)
Year 2007 X Treatment	186.37	0.49	5.18***	0.23	5.02***
	(250.61)	(0.71)	(1.98)	(1.35)	(1.69)
Year 2008 X Treatment	423.59	1.33	4.21*	0.78	4.69**
	(309.02)	(0.98)	(2.19)	(1.45)	(1.99)
Year 2009 X Treatment	568.72**	1.60	3.94*	1.23	4.30**
	(256.24)	(1.03)	(2.09)	(1.34)	(1.85)
p-value for test that all pre-2006 terms jointly equal 0	0.51	0.82	0.32	0.03	0.87
N	50,579	50,579	50,579	50,579	50,579

Panel B: OOP Price (per Day Supplied)

	Total MME (1)	High Dose Opioids (2)	Low Dose Opioids (3)	Extended Release Opioids (4)	Immediate Release Opioids (5)
Year 2000 X Treatment	-0.01	-0.33	0.57	0.00	0.53
	(0.02)	(0.84)	(0.50)	(0.57)	(0.57)
Year 2001 X Treatment	-0.01	5.16	-0.12	2.53	-0.26
	(0.03)	(3.68)	(0.72)	(1.89)	(0.66)
Year 2002 X Treatment	-0.01	1.09	0.38	0.82	0.24
	(0.01)	(3.97)	(0.55)	(3.17)	(0.43)

Year 2003 X Treatment	-0.01 (0.01)	5.68* (3.26)	-0.30 (0.46)	1.21 (2.45)	0.26 (0.40)
Year 2004 X Treatment	-0.01 (0.02)	-0.41 (5.40)	0.65 (0.47)	-2.07 (4.21)	1.16** (0.55)
Year 2006 X Treatment	-0.03 (0.02)	-4.22 (6.96)	0.23 (0.55)	-1.50 (4.59)	-0.49 (0.52)
Year 2007 X Treatment	-0.01 (0.01)	5.79 (5.41)	-1.25** (0.63)	1.25 (2.24)	-0.97 (0.62)
Year 2008 X Treatment	-0.03** (0.01)	0.33 (0.26)	-0.57 (0.53)	-1.02 (0.79)	-0.33 (0.50)
Year 2009 X Treatment	-0.02*** (0.01)	2.01** (0.89)	-0.70 (0.56)	0.05 (0.49)	-0.34 (0.69)
p-value for test that all pre-2006 terms jointly equal 0	0.88	0.39	0.53	0.80	0.29
N	1,664	308	1,356	223	1,441

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Table displays the coefficient on the interaction of the treatment group indicator and each year indicator. The year 2005 is omitted as the base year. Regressions in Panel A control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. Regressions in Panel B include a treatment group indicator and year fixed effects. Data are weighted by 2003 level of utilization of the NDC.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 7. Falsification Tests for Impact of Part D on Prescription Painkiller Utilization (Days Supplied)

	"Treatment" as Ages 45-54 (1)	"Treatment" as Ages 35-44 (2)	"Treatment" as Ages 25-34 (3)	"Treatment" as Ages 18-24 (4)
All Painkillers	0.99	0.57	0.18	-0.16
	(1.72)	(1.39)	(1.40)	(1.42)
Opioids	1.22	-0.79	-1.23	-1.43*
	(0.98)	(0.77)	(0.77)	(0.76)
Non-Opioid Painkillers	0.22	1.25	1.23	1.28
	(1.43)	(1.15)	(1.13)	(1.18)
N	74,703	77,310	74,883	63,708

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. The control group consists of individuals aged 55-64, and the column header provides the definition of the "treatment" group. Each cell displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table 1- 8. DD Results with Two Post Periods for Impact of Part D on Prescription Painkiller Utilization (Days Supplied)

	Treatment X 2004-05 (1)	Treatment X Post-2006 (2)	N (3)
All Painkillers	4.03* (2.40)	5.76** (2.38)	50,579
Opioids	1.63 (1.17)	5.39*** (1.30)	50,579
Non-Opioid Painkillers	2.43 (2.21)	0.88 (2.10)	50,579

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Column 1 displays the coefficient on the interaction of the treatment group indicator and the 2004-05 indicator. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 9. Sensitivity Analyses for Impact of Part D on Prescription Painkiller Utilization (Days Supplied)

Panel A: Alternative Specifications

	No Demographic Controls (1)	Omit Years 2004-05 (2)	Omit Ages 63-64 (3)	Alternative Treatment (4)	Treatment X Year FE (5)	Control for Health Status (6)	Treatment X Year FE and Control for Health Status (7)
All Painkillers	4.31*	5.90**	4.82**	3.59	4.85	3.87*	4.53
	(2.26)	(2.39)	(2.35)	(2.28)	(4.11)	(2.27)	(4.00)
Opioids	4.72***	5.38***	4.95***	3.11***	4.50**	4.53***	4.30**
	(1.21)	(1.30)	(1.20)	(1.18)	(2.19)	(1.23)	(2.13)
Non-Opioid Painkillers	0.06	1.02	0.29	0.48	1.27	-0.18	1.12
	(2.00)	(2.11)	(2.11)	(2.05)	(3.50)	(2.02)	(3.47)
N	50,579	40,539	45,700	47,929	50,579	50,579	50,579

Panel B: Include Additional Years of Data

	Years 1996-2009 (1)	Years 2000-2015 (2)	Years 1996-2015 (3)
All Painkillers	5.88***	7.73***	9.37***
	(2.14)	(1.86)	(1.65)
Opioids	5.34***	7.23***	7.86***
	(1.17)	(0.97)	(0.91)
Non-Opioid Painkillers	1.18	1.35	2.55*
	(1.86)	(1.71)	(1.50)
N	65,363	87,899	102,683

Source: Author's calculations based on Medical Expenditure Panel Survey. In Panel A, years of analysis are restricted to 2000 to 2009. Each cell displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Unless otherwise specified, regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 10. DD Results for Impact of Part D on Number of Painkiller Prescriptions (Number of Prescriptions)

	Utilization (Prescriptions)		
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)
Painkillers			
All Painkillers	1.96	0.11 (0.11)	-
Opioids	0.73	0.18*** (0.07)	24.7%
Non-Opioid Painkillers	1.23	-0.07 (0.07)	-
Opioids, by Dosage			
High Dose Opioids	0.15	0.01 (0.03)	-
Low Dose Opioids	0.59	0.17*** (0.06)	28.8%
Opioids, by Release			
Extended Release Opioids	0.12	0.05 (0.03)	
Immediate Release Opioids	0.62	0.13** (0.05)	21.0%
N		50,579	

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, column 3 displays percent change from pre-2006 mean.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 11. Heterogeneous Effects for Impact of Part D on Prescription Painkiller Utilization by Household Income

	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)
Less than 125% FPL			
All Painkillers	51.70	8.31 (5.66)	-
Opioids	16.50	5.54 (3.44)	-
Non-Opioid Painkillers	38.06	3.29 (4.96)	-
N		9,259	
125-400% FPL			
All Painkillers	41.65	6.37* (3.61)	15.3%
Opioids	9.47	7.88*** (2.08)	83.2%
Non-Opioid Painkillers	33.49	-0.77 (3.08)	-
N		21,191	
Greater than 400% FPL			
All Painkillers	31.45	0.10 (2.96)	-
Opioids	6.58	0.53 (1.46)	-
Non-Opioid Painkillers	25.93	-0.55 (2.59)	-
N		20,129	

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, column 3 displays percent change from pre-2006 mean.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 12. Robustness Checks for Impact of Part D on Prescription Painkiller Utilization

	Ages 50-79 (1)	Ages 51-78 (2)	Ages 52-77 (3)	Ages 53-76 (4)	Ages 54-75 (5)	Ages 55-74 (6)	Ages 56-73 (7)	Ages 57-72 (8)	Ages 58-71 (9)	Ages 59-70 (10)	Ages 60-69 (11)
All Painkillers	4.22**	4.01**	4.11**	4.51**	4.75**	4.33*	4.45*	5.00**	4.61*	3.45	2.17
	(1.79)	(1.84)	(1.91)	(2.01)	(2.17)	(2.30)	(2.38)	(2.48)	(2.66)	(2.85)	(3.06)
Opioids	4.71***	4.64***	4.65***	4.78***	4.98***	4.81***	4.91***	5.24***	5.06***	4.28***	3.11*
	(1.00)	(1.03)	(1.06)	(1.09)	(1.15)	(1.23)	(1.29)	(1.37)	(1.44)	(1.53)	(1.71)
Non-Opioid Painkillers	0.13	-0.07	-0.02	0.18	0.39	0.02	0.15	0.39	0.16	-0.41	-0.80
	(1.60)	(1.63)	(1.72)	(1.81)	(1.94)	(2.04)	(2.08)	(2.15)	(2.30)	(2.45)	(2.62)
N	79,033	73,350	67,583	61,851	56,231	50,579	45,128	39,653	34,221	29,118	24,195

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to ages defined in the column header. Each cell displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table 1- 13. DD Results for Impact of Part D on Any Purchase of Prescription Painkillers

	Utilization (Any Purchase)		
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)
Painkillers			
All Painkillers	0.35	0.01 (0.01)	-
Opioids	0.17	0.01 (0.01)	-
Non-Opioid Painkillers	0.26	-0.01 (0.01)	-
N		50,579	

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, column 3 displays percent change from pre-2006 mean.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table 1- 14. Impact of Part D on Free Samples of Prescription Painkillers

	Number of Free Sample Prescriptions		
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)
All Drugs	0.96	-0.20*** (0.07)	-20.8%
All Painkillers	0.09	-0.02* (0.01)	-22.2%
Opioids	0.01	-0.00 (0.00)	-
Non-Opioid Painkillers	0.08	-0.02* (0.01)	-25.0%
N		50,579	

Source: Author's calculations based on Medical Expenditure Panel Survey 2000 to 2009. Sample is restricted to adults aged 55 to 74. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include age fixed effects and year fixed effects. Data are adjusted by MEPS survey weights, and standard errors account for the complex design of the MEPS. For statistically significant point estimates, column 3 displays percent change from pre-2006 mean.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 15. DD Results for Impact of Part D on Prices of OTC Painkillers

	Price (per Day Supplied)		
	Pre-2006 Mean (1)	DD Coefficient (2)	Percent Change (3)
OTC Painkillers	0.37	0.06 (0.06)	-
N		335,060	

Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2009. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for householder's sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include household fixed effects and year fixed effects. Data are adjusted by Nielsen survey weights, and standard errors are clustered on household.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table 1- 16. Event Study Results for Impact of Part D on Utilization of OTC Painkillers

	Utilization (Days Supplied) (1)
Year 2004 X Treatment	2.05** (0.91)
Year 2006 X Treatment	3.41*** (0.93)
Year 2007 X Treatment	-3.67*** (0.96)
Year 2008 X Treatment	-4.01*** (1.00)
Year 2009 X Treatment	-7.86*** (1.04)
N	335,060

Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2009. Column 1 displays the coefficient on the interaction of the treatment group indicator and each year indicator. The year 2005 is omitted as the base year. Regressions control for householder's sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include household fixed effects and year fixed effects. Data are adjusted by Nielsen survey weights, and standard errors are clustered on household.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 17. Sensitivity Analyses for Impact of Part D on Utilization of OTC Painkillers

	No Controls (1)	No Weights (2)	No Household FE (3)	Any Purchase Outcome (4)
OTC Painkillers	-3.46***	-3.46***	0.18	-0.01**
	(0.98)	(0.72)	(1.24)	(0.01)
N	335,060	335,060	335,060	335,060

Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2009. Table displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Results should be compared with those in Table 1-8. The outcome variable is "number of days supplied of OTC painkillers" for columns 1-3 and "any painkiller purchased" for column 4 (pre-2006 mean for "any painkiller purchased" is 0.79). Unless otherwise specified, regressions control for householder's sex, marital status, household income, educational attainment, race/ethnicity, and Census region, and include household fixed effects and year fixed effects. Data are adjusted by Nielsen survey weights, and standard errors are clustered on household.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1- 18. Heterogeneous Effects for Impact of Part D on OTC Painkiller Utilization by Household Income

	Pre-2006 Mean (1)	DD Estimate (2)	Percent Change (3)
Less than 125%			
OTC Painkillers	61.28	-1.97 (3.75)	-
N		20,031	
125 to 400%			
OTC Painkillers	75.72	-2.50* (1.33)	-3.3%
N		153,486	
Greater than 400%			
OTC Painkillers	80.33	-5.81*** (1.96)	-7.2%
N		161,539	

Source: Author's calculations based on Nielsen Household Consumer Panel 2004 to 2009. Column 1 displays the pre-2006 mean for the treatment group. Column 2 displays the coefficient on the interaction of the treatment group indicator and the post-2006 indicator. Regressions control for householder's sex, marital status, educational attainment, race/ethnicity, and Census region, and include household fixed effects and year fixed effects. Data are adjusted by Nielsen survey weights, and standard errors are clustered on household.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendices for Chapter 2

2-A. Review of Medicare Part D Literature

Appendix Table 2- 1. Literature on the Impact of the Introduction of Medicare Part D[38]

Paper	Outcomes Studied	Methods/Data	Results
Outcome: Prescription Drug Coverage			
Heiss et al. (2006). Who Failed To Enroll In Medicare Part D, And Why? Early Results. *Health Affairs.*	Drug coverage by socio-demographic characteristics	Panel/cohort; Retirement Perspectives Survey	After the first year of Part D, only 7.4% of elderly Americans remained uninsured. The remaining uninsured have better self-assessed health, use fewer prescriptions, and lower cognitive impairment scores. Part D was largely successful in enrolling vulnerable subpopulations –poor health, low income, or cognitive impairment.

[38] This review covers studies that evaluate impacts of the 2006 introduction of Medicare Part D. There are also several studies that leverage the Part D coverage gap (donut hole) to understand the effect of OOP prices on drug utilization and other outcomes. This table does *not* cover the literature on the Part D coverage gap.

Levy & Weir. (2009). Take-up of Medicare Part D: Results From the Health and Retirement Study. *Journal of Gerontology: Social Sciences.*	Drug coverage; Part D take up; Source of coverage	Panel/cohort; HRS	Drug uninsurance reduced from 24% in 2004 to 7% in 2006. The remaining uninsured use fewer prescriptions and have lower OOP spending. Take up of Part D was high (60%) among uninsured population.
Outcome: Drug Utilization, Nonadherence, and OOP Spending			
Hall et al. (2007). Transition to Medicare Part D: An Early Snapshot of Barriers Experienced by Younger Dual Eligibles with Disabilities. *American Journal of Managed Care.*	Impact of transition from Medicaid to Part D on drug utilization among non-elderly disabled adults	Descriptive; Survey of Kansas Medicaid beneficiaries	Transition to Part D led to some access problems for the disabled. 20% of respondents reported difficulty obtaining medications; 13% were required to switch medications; 8% stopped taking at least one drug.
Lichtenberg & Sun. (2007). The Impact Of Medicare Part D On Prescription Drug Use By The Elderly. *Health Affairs.*	Drug utilization; OOP spending; Crowd-out	Diff-in-diff; Walgreens	OOP costs reduced by 18%' drug utilization increased by 13%. Crowd-out rate was 72%: for every 7 prescriptions paid for by government, only 2 were new and 5 were crowded out from private insurance.
West et al. (2007). Medication Access and Continuity: The Experiences of Dual-Eligible Psychiatric Patients During the First 4 Months of the Medicare Prescription Drug Benefit. *American Journal of Psychiatry.*	Impact of transition from Medicaid to part D on drug utilization among dual-eligible psychiatric patients	Descriptive; Survey of psychiatrists	Access problems for psychiatric patients. 53% had at least one medication access problem in 2006. Among those with medication access problems, 27% experienced adverse clinical event; 20% had ED visit.
Chen et al. (2008). The Impact of Medicare Part D on Psychotropic Utilization and Financial Burden for Community-Based Seniors. *Psychiatric Services.*	Utilization of psychotropic drugs; OOP spending	Interrupted time series; Walgreens	Proportion of OOP payments in total pharmacy reimbursements decreased 18% for antidepressants, decreased 21% of antipsychotics, & increased 19% for benzodiazepines (benzodiazepines excluded from formulary). Prescriptions increased by 7% for antidepressants, increased by 18% for antipsychotics, and decreased 5% for benzodiazepines.
Ketcham & Simon. (2008). Medicare Part D's Effects on Elderly Drug Costs and Utilization. *American Journal of Managed Care.*	Drug utilization; OOP spending	Diff-in-diff; Wolters Kluwer Health (prescription records)	OOP costs per day supplied reduced by 22%; drug utilization increased by 5%; implied elasticity is -0.22.

Shrank et al. (2008). The Effect of Transitioning to Medicare Part D Drug Coverage in Seniors Dually Eligible for Medicare and Medicaid. *Journal of the American Geriatrics Society.*	Drug utilization; OOP spending; Medication switching among dual eligibles	Interrupted time series; Pharmacy chain data	Reduction in copays for all drugs studied, except increases in benzodiazepine copays. 3 times greater rate of switching medications for proton pump inhibitors, but no change for other medications. No change in utilization of all study drugs, including the uncovered benzodiazepines.
Yin et al. (2008). The Effect of the Medicare Part D Prescription Benefit on Drug Utilization and Expenditures. *Annals of Internal Medicine.*	Drug utilization; OOP spending	Diff-in-diff; Walgreens	OOP expenditures per day supplied reduced by 9% and drug utilization increased by 1% in the first half of 2006. After enrollment stabilized, OOP expenditures decreased by 13% and utilization increased by 6%.
Zhang et al. (2008). The Impact of the Medicare Part D Prescription Benefit on Generic Drug Use. *Journal of General Internal Medicine.*	Generic drug utilization; OOP spending	Diff-in-diff; Walgreens	Modest decrease in use of generic drugs among non-enrollees (odds ratio was 0.95), driven by antihyperlipidemics, antihistamines, NSAIDs, and beta blockers.
Briesacher et al. (2009). Nursing Home Residents and Enrollment in Medicare Part D. *Journal of the American Geriatrics Society.*	Enrollment, payment source, and drug utilization among nursing home residents	Interrupted time series; Large long-term care pharmacy provider	By 2006, 97% of nursing home residents had drug coverage. Proportion of prescription drugs paid out-of-pocket decreased from 11% in 2005 to 8% in 2006. Average monthly prescription use per resident in 2006 decreased by half a prescription relative to 2005 levels.
Das-Douglas et al. (2009). Implementation of the Medicare Part D Prescription Drug Benefit is Associated with Antiretroviral Therapy Interruptions. *AIDS and Behavior.*	ARV utilization among HIV-infected homeless	Descriptive; Research on Access to Care in the Homeless	Part D increased consumer cost sharing for ARV treatment. Odds of ARV interruptions were 6 times higher among those with Part D coverage; majority cited cost as primary barrier.
Huskamp et al. (2009). Part D and Dually Eligible Patients With Mental Illness: Medication Access Problems and Use of Intensive Services. *Psychiatric Services.*	Utilization of intensive mental health services among dual eligibles	Propensity score weighting; Survey of psychiatrists	44% of dual-eligible patients reported a problem accessing medications. The likelihood of having an emergency room visit was significantly higher for those who experienced a problem compared to those who did not.
Joyce et al. (2009). Medicare Part D After 2 Years. *American Journal of Managed Care.*	Drug utilization; OOP spending	Interrupted time series; MCBS	90% of seniors had coverage at least as generous as standard Part D by 2008. 16% annual decrease in out-of- pocket spending and 7% increase in the number of prescriptions.

Madden et al. (2009). Cost-Related Medication Nonadherence and Spending on Basic Needs Following Implementation of Medicare Part D. *JAMA.*	Cost-related medication nonadherence	Diff-in-diff; MCBS	Decline in cost-related medication nonadherence. Sicker disabled beneficiaries experienced lagged improvements.
Schneeweiss et al. (2009). The Effect Of Medicare Part D Coverage On Drug Use And Cost Sharing Among Seniors Without Prior Drug Benefits. *Health Affairs.*	Utilization and OOP spending on clopidogrel, warfarin, PPIs, & statins among previously uninsured seniors	Interrupted time series; Prescriptions dispensed from 3 pharmacy chains	55% of previously uninsured patients received coverage after Part D in 2006 (mostly initiated in first half of year). Utilization of statins increased by 22%, PPIs increased by 37%, clopidogrel increased by 11%, warfarin increased by 3%. OOP spending decreased by 52% for statins, 56% for PPIs, 58% for clopidogrel, and 37% for warfarin.
Zivin et al. (2009). Cost-Related Medication Nonadherence Among Beneficiaries With Depression Following Medicare Part D. *American Journal of Geriatric Psychiatry.*	Cost-related medication nonadherence	Interrupted time series; MCBS	Nonadherence did not decline among beneficiaries with depressive symptoms compared with beneficiaries without depressive symptoms. Part D did not improve this disparity.
Basu et al. (2010). Impact of Medicare Part D on Medicare-Medicaid Dual-Eligible Beneficiaries' Prescription Utilization and Expenditures. *Health Services Research.*	Drug utilization and OOP spending for elderly dual eligibles	Diff-in-diff; Walgreens	Compared to near-elderly patients with Medicaid, elderly patients with Medicaid experienced no significant change (positive or negative) in OOP spending, days supplied, or number of prescriptions due to Part D.
Millett et al. (2010). Impact of Medicare Part D on Seniors' Out-of-pocket Expenditures on Medications. *Archives of Internal Medicine.*	OOP expenditures	Panel/cohort; MEPS	OOP expenditures decreased by 32% for all Medicare beneficiaries, by 49% for beneficiaries without previous drug coverage who enrolled in Part D, by 32% for beneficiaries who did not enroll, and did not change significantly for dual eligibles.
Mott et al. (2010). Effects of Medicare Part D on Drug Affordability and Use: Are Seniors with Prior High Out-of-Pocket Drug Spending Affected More? *Research in Social and Administrative Pharmacy*.	Drug utilization and expenditures for patients with different levels of pre-Part D drug use	Diff-in-diff; Regional supermarket pharmacy chain data	18% reduction in OOP spending; 4% increase in drug use. Reduction in OOP spending was greatest for those with highest pre-Part D utilization.

Zhang et al. (2010). How the Medicare Part D Drug Benefit Changed the Distribution of Out-of-Pocket Pharmacy Spending Among Older Beneficiaries. *Journals of Gerontology Series B: Psychological Sciences and Social Sciences.*	Distribution of OOP spending among elderly	Diff-in-diff; Large Medicare Advantage insurer in Pennsylvania	OOP spending reduced by 13% among those without previous coverage. However, those with the highest drug spending still pay a substantial share of their drug costs OOP.
Briesacher et al. (2011). Medicare Part D and Changes in Prescription Drug Use and Cost Burden. *Medical Care.*	Drug utilization and OOP drug costs	Panel/cohort; MCBS	Prescription fills increased by 1.8 in 2006 and 3.4 in 2007 (above pre-Part D increases of 0.9 fills per year). OOP drug costs decreased by $143 in 2006 and $148 in 2007 (above pre-Part D increases of $12 per year).
Donohue et al. (2011). Impact of Medicare Part D on Antidepressant Treatment, Medication Choice, and Adherence Among Older Adults With Depression. *American Journal of Geriatric Psychiatry*.	Antidepressant utilization among older adults with depression	Diff-in-diff; Large Medicare Advantage insurer in Pennsylvania	Increased odds of any antidepressant use among those who previously lacked coverage, but odds of use did not change among those with limited prior coverage.
Engelhardt. (2011). Prescription Drug Insurance Coverage, Drug Utilization, and Cost-Related Non-Adherence: Evidence from the Medicare Part D Expansion. Working Paper.	Drug utilization, Cost-related non-adherence	Fixed effects/Panel; HRS Prescription Drug Study	Gaining coverage leads to a 15% increase in utilization and a 20-50% decrease in incidence of cost-related non-adherence. Overall drug coverage increased by 10 percentage points; crowd-out of 73%.
Engelhardt & Gruber. (2011). Medicare Part D and the Financial Protection of the Elderly. *AEJ: Economic Policy.*	Public vs private drug spending; Crowd-out	Diff-in-diff; Instrumental variables; MEPS	75% crowd-out of both prescription drug insurance coverage and expenditures among the elderly. Large reductions in OOP spending on average, but the bulk of these accrue to a small proportion of the elderly (those with highest spending).
Zhang et al. (2011). The Impact of Medicare Part D on Medication Treatment of Hypertension. *Health Services Research.*	Utilization of antihypertensive medications among seniors with hypertension	Diff-in-diff; Large Medicare Advantage insurer in Pennsylvania	Increased antihypertensive use and use of ARBs over less expensive alternatives.
Mahmoudi & Jensen. (2014). Has Medicare Part D Reduced Racial/Ethnic Disparities in Prescription Drug Use and Spending? *Health Services Research.*	Racial/ethnic disparities in drug utilization and OOP spending	Triple differences; MEPS	Part D reduced disparities in drug utilization between Hispanic and Whites, but increased disparities in drug spending between Blacks and Whites.

Zimmer. (2015). The Effect of Medicare Part D on Prescription Drug Composition and Demand. *Journal of Economic Studies.*	Number of therapeutic classes for which drugs were prescribed; Usage of drugs for specific medical conditions that typically receive drug-intensive therapies	Diff-in-diff; MEPS	Number of therapeutic classes to which seniors receive drugs increased by approximately four classes. Increased usage of drugs used to treat upper respiratory disease, hypertension, and diabetes.
Alpert. (2016). The Anticipatory Effects of Medicare Part D on Drug Utilization. *Journal of Health Economics.*	Drug utilization after announcement but before implementation of Part D	Diff-in-diff; MCBS; MEPS	People reduced drug utilization for chronic (but not acute) drug in anticipation of Part D's implementation. Accounting for this anticipatory effect reduces the estimated total treatment effect of Part D.
Hu, Decker, & Chou. (2017). The Impact of Health Insurance Expansion on Physician Treatment Choice: Medicare Part D and Physician Prescribing. *International Journal of Health Economics and Management*.	Physician prescribing decisions	Regression discontinuity; NAMCS	32% increase in the number of prescription drugs prescribed or continued per visit and a 46% increase in the number of generic drugs prescribed or continued for the elderly after Part D.
Outcome: Drug Prices			
Duggan & Scott Morton. (2010). The Effect of Medicare Part D on Pharmaceutical Prices and Utilization. *American Economic Review.*	Prescription drug prices	Exploit variation across drug classes in their pre-Part D Medicare market shares; IMS Health; MEPS	Part D plans successfully negotiated lower price increases for Part D enrollees – approximately 20% lower than they otherwise would have been. Insured customer is more price elastic than uninsured customer, because in Part D, insurance is bundled with group purchasing and formulary implementation.
Duggan & Scott Morton. (2011). The Medium-Term Impact of Medicare Part D on Pharmaceutical Prices. *American Economic Review.*	Prescription drug prices	Exploit variation across drug classes in their pre-Part D Medicare market shares; IMS Health; MEPS	Part D plans successfully negotiated lower prices through the first four years of the program.

Lakdawalla & Yin. (2015). Insurers' Negotiating Leverage and the External Effects of Medicare Part D. *Review of Economics and Statistics*.	Prescription drug prices negotiated in the non-Medicare commercial market	Instrumental variables using insurers' geographic exposure to total potential Part D enrollment; Pharmacy chain data	Part D lowered prices for commercial enrollees by 3.7 percentage. The external commercial market savings amount to $1.5 billion per year.
Outcome: Hospitalizations and Other Non-Drug Medical Spending			
Zhang et al. (2009). The Effect of Medicare Part D on Drug and Medical Spending. *New England Journal of Medicine.*	Drug spending; Other medical spending (ED, hospitalization, outpatient, etc)	Propensity score weighting; Large Medicare Advantage insurer in Pennsylvania	Part increased monthly drug spending by $41, increased the use of lipid-lowering and antidiabetic medications, and reduced non-drug monthly medical expenditures by $33.
Afendulis et al. (2011). The Impact of Medicare Part D on Hospitalization Rates. *Health Services Research.*	Hospitalization rates for 8 conditions sensitive to drug adherence (diabetes, complications, stroke, AMI, etc)	Triple differences; HCUP	Part D reduced overall rate of hospitalizations by 20.5 per 10,000, representing about 42,000 admissions.
Liu et al. (2011). The Impact of Medicare Part D on Out-of-Pocket Costs for Prescription Drugs, Medication Utilization, Health Resource Utilization, and Preference-Based Health Utility. *Health Services Research.*	Drug utilization; ED utilization; Hospitalizations; Preference-based health utility	Diff-in-diff; MEPS	Part D reduced drug costs by $180 per year and increased utilization by 2 prescriptions per year. No significant change in ED use, hospitalizations, or preference-based health utility in the first year of implementation.
McWilliams et al. (2011). Implementation of Medicare Part D and Nondrug Medical Spending for Elderly Adults With Limited Prior Drug Coverage. *JAMA.*	Nondrug medical spending	Diff-in-diff; HRS & Medicare claims	Compared to beneficiaries who previously had generous drug coverage, those who had limited prior coverage experienced reduction in nondrug medical spending, driven by inpatient and skilled nursing facility care. No change in outpatient spending.
Kaestner & Khan. (2012). Medicare Part D and Its Effect on the Use of Prescription Drugs and Use of Other Health Care Services of the Elderly. *Journal of Policy Analysis & Management.*	Drug utilization; Outpatient and inpatient services	Instrumental variables (using predicted likelihood of being uninsured pre-2006); MCBS	Prescription drug insurance increases number of prescriptions by 30% per year. No impact on outpatient and inpatient services.

Ayyagari, Shane, & Wehby. (2017). The Impact of Medicare Part D on Emergency Department Visits. *Health Economics.*	ED utilization	Diff-in-diff; MEPS	Declines in the number of ED visits for non-emergency care but not for emergency care, suggesting that Part D may have led to better management of health and reduced unnecessary use of EDs.
Kaestner et al. (2017). Effects of Prescription Drug Insurance on Hospitalization and Mortality: Evidence from Medicare Part D. *Journal of Risk and Insurance.*	Inpatient hospital admissions; Mortality	Diff-in-diff; Instrumental variables; Medicare claims (fee for service only); MCBS	Gaining prescription drug insurance through Part D caused a 4% decrease in hospital admissions, a 2-5% decrease in Medicare inpatient payments, and a 10-15% decrease in inpatient charges. No significant effect on mortality.
Outcome: Health Outcomes			
Hanlon et al. (2013). Racial Differences in Antilipemic Use and Lipid Control in High Risk Older Adults Post Medicare Part D. *American Heart Journal.*	Utilization of antilipemics; Cholesterol levels	Descriptive; Healthy Aging and Body Composition	No significant impact on cholesterol levels, in spite of increased use of statins and other antilipemic drugs.
Ayyagari & Shane. (2015). Does Prescription Drug Coverage Improve Mental Health? Evidence from Medicare Part D. *Journal of Health Economics.*	Depressive symptoms	Diff-in-diff; HRS	Reduced number of depressive symptoms by 15% and likelihood of experiencing 3+ depressive symptoms by 21%.
Ayyagari. (2016). Prescription Drug Coverage and Chronic Pain. *International Journal of Health Economics and Management.*	Pain; activity limitations due to pain	Diff-in-diff; HRS	5.3% improvement in activity limitations due to pain.
Chen, Lin, & Seo. (2018). Medicare Part D Implementation and Associated Health Impact Among Older Adults in the United States. *International Journal of Health Services.*	Self-rated health; Mental health; Activities of daily living	Diff-in-diff; HRS Prescription Drug Study 2005-07	Increased self-rated health. No change in mental health or activities of daily living impairment.
Outcome: Mortality			
Dunn & Shapiro. (2017). Does Medicare Part D Save Lives? *American Journal of Health Economics*.	Mortality by condition	Diff-in-diff (by county); Mortality files; MCBS	Cardiovascular mortality decreased in counties most affected by Part D. Up to 26,000 more individuals were alive in mid-2007 because of Part D.

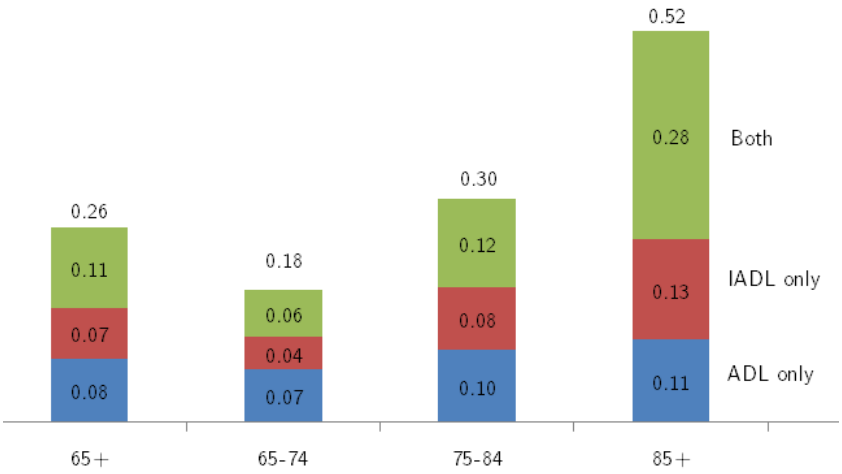
Huh & Reif. (2017). Did Medicare Part D Reduce Mortality? *Journal of Health Economics.*	Mortality by condition	Regression discontinuity (age 66 vs 64); Mortality files; MEPS	Part D reduced elderly mortality by 2.2% annually, primarily driven by a reduction in cardiovascular mortality. No effect on cancer.
Kaestner et al. (2017). Effects of Prescription Drug Insurance on Hospitalization and Mortality: Evidence from Medicare Part D. *Journal of Risk and Insurance.*	Inpatient hospital admissions; Mortality	Diff-in-diff; Instrumental variables; Medicare claims (fee for service only); MCBS	Gaining prescription drug insurance through Part D caused a 4% decrease in hospital admissions, a 2-5% decrease in Medicare inpatient payments, and a 10-15% decrease in inpatient charges. No significant effect on mortality.
Outcome: Labor Market			
Moulton, Diebold, & Scott. (2016). The Impact of Medicare Part D on Self-Employment. *Research on Aging.*	Self-employment	Diff-in-diff; ACS	Part D increased likelihood of self-employment by 5%.
Wettstein. (2019). Retirement Lock and Prescription Drug Insurance: Evidence from Medicare Part D. *AEJ: Economic Policy.*	Labor supply; Full-time work; Part-time work	Triple differences; HRS	Compared to those who would be covered after age 65, those with retiree drug benefits only up to age 65 decreased full-time work by 8 percentage points, of which 70% was due to transitions to part-time work.
Outcome: Other			
Lakdawalla & Sood. (2009). Innovation and the Welfare Effects of Public Drug Insurance. *Journal of Public Economics*.	Social welfare	Welfare model	Medicare Part D generates $3.5 billion of annual static deadweight loss reduction, and at least $2.8 billion of annual value from extra innovation. These two components alone cover 87% of the social cost of publicly financing the benefit.
Blume-Kohout & Sood. (2013). Market Size and Innovation: Effects of Medicare Part D on Pharmaceutical Research and Development. *Journal of Public Economics.*	Pharmaceutical R&D (number of drugs entering preclinical and clinical development)	Exploit variation across drug classes in their pre-Part D Medicare market shares; Pharmaprojects	Part D led to significant increases in pharmaceutical R&D for therapeutic classes with higher Medicare market share.

Lakdawalla, Sood, & Gu. (2013). Pharmaceutical Advertising and Medicare Part D. *Journal of Health Economics.*	Pharmaceutical advertising	Exploit variation across drug classes in their pre-Part D Medicare market shares; IMS Advertising; MEPS	Part D coincides with a 14-19% increase in total advertising expenditures. The additional advertising raised utilization among non-elderly patients outside the Part D program by about 3.6%.
Ayyagari & He. (2016). Medicare Part D and Portfolio Choice. *American Economic Review: Papers and Proceedings.*	Share of financial wealth invested in risky assets (stocks, mutual funds, etc)	Diff-in-diff; HRS	The share of financial wealth invested in risky assets increased by 3.2 percentage points, a 7.2 percent increase relative to the pre-2003 mean.
Powell, Pacula, & Taylor. (2016). How Increasing Medical Access to Opioids Contributes to the Opioid Epidemic: Evidence from Medicare Part D. *RAND Working Paper, Number 1169.*	Opioid-related substance abuse treatment admissions and opioid-related mortality among the non-elderly	Diff-in-diff (by state); Mortality; ARCOS; TEDS	10% increase in opioid medical supply leads to a 7.4% increase in opioid-related deaths among the Medicare-ineligible population, suggesting substantial diversion from medical markets.
Asfaw. (2019). The Effect of Prescription Drug Insurance on Health Behavior: Evidence from Medicare Part D. *Health Economics.*	Health behaviors – Exercise, Smoking, BMI	Difference in regression discontinuity comparing 65-69 year olds vs. 60-64 year olds; NHIS; MEPS	Decreased moderate exercise at the extensive and intensive margins. Increased probability of being overweight. No change in vigorous exercise, muscle-strengthening exercise, smoking, BMI, probability of healthy weight, and probability of obesity.

2-B. Appendix 2 Figures

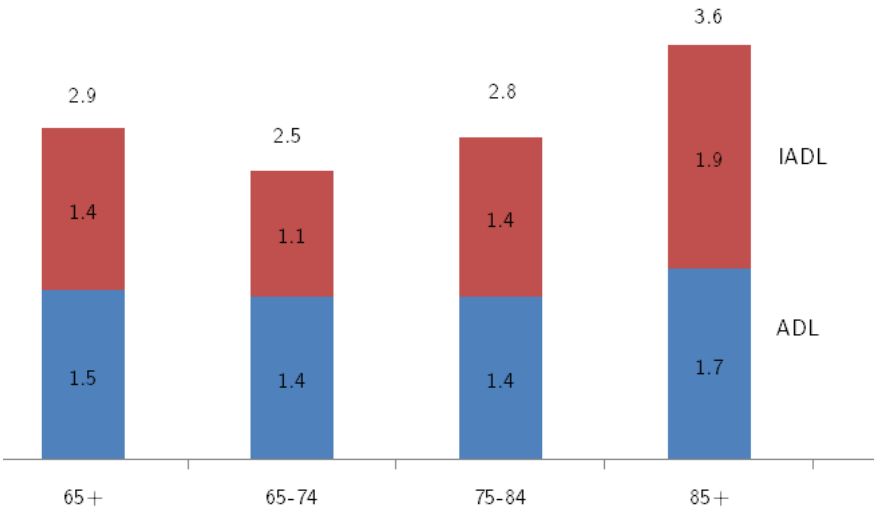
Appendix Figure 2- 1. Functional Limitations among the Elderly

A. Proportion of Elderly Adults with Functional Limitations



Source: Author's calculations based on HRS 1996-2012. Sample is restricted to adults aged 65 and over (N=91,038). Figure displays proportion of adults who report "at least some difficulty" with the activity. Estimates include HRS sampling weights.

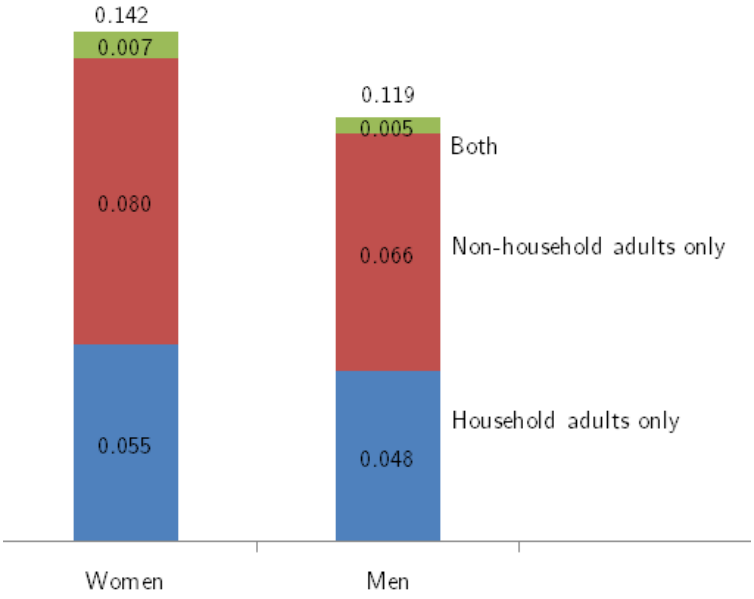
B. Number of Limitations among those with Any



Source: Author's calculations based on HRS 1996-2012. Sample is restricted to adults aged 65 and over who have at least one functional limitation (N=24,418). Figure displays average number of functional limitations among adults who report at least one limitation. Estimates include HRS sampling weights.

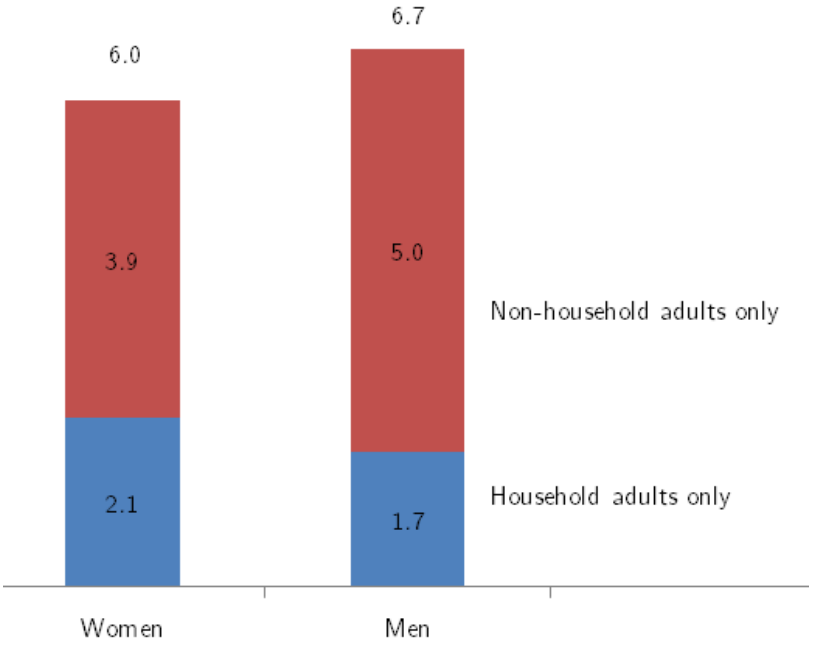
Appendix Figure 2- 2. Informal Caregiving among the Nonelderly

A. Proportion of Nonelderly Adults who Provide Caregiving



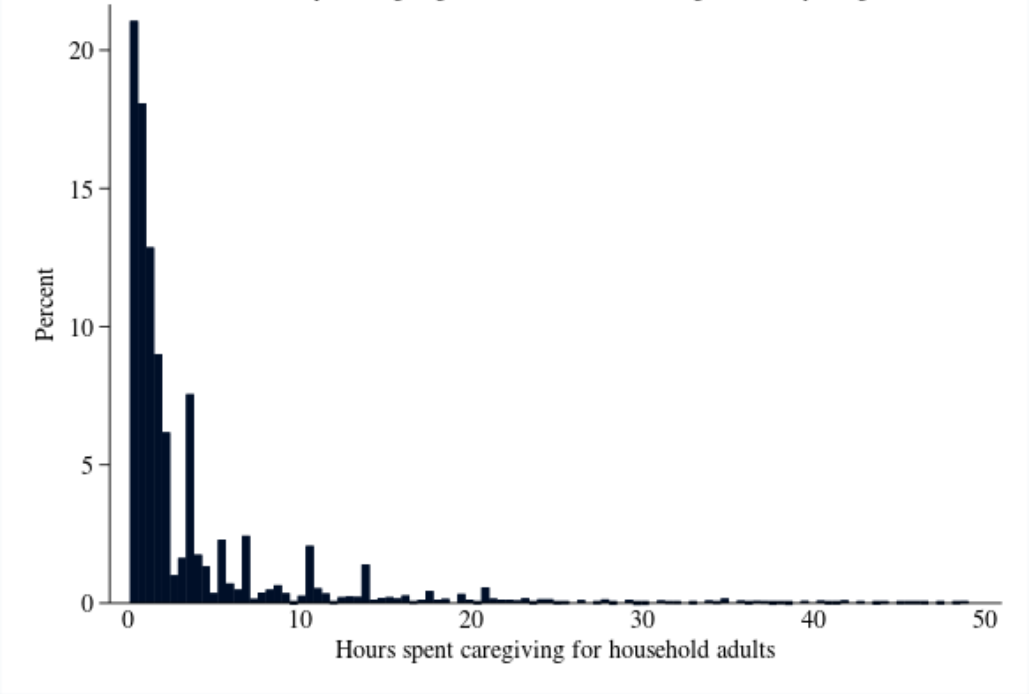
Source: Author's calculations based on ATUS 2003-13. Sample is restricted to adults aged 27-64 (N=101,423). Estimates include ATUS sampling weights.

B. Caregiving Hours Per week among non-Elderly Caregivers



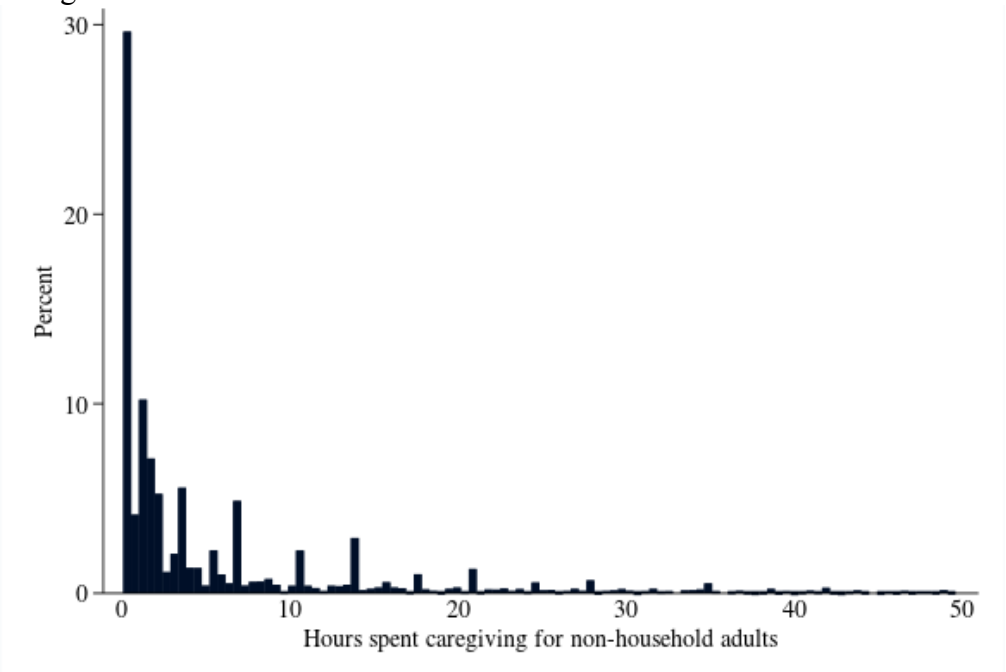
Source: Author's calculations based on ATUS 2003-13. Sample is restricted to adults aged 27-64 who spend greater than 0 hours per week caregiving for household or non-household adults (N=12,876). Estimates include ATUS sampling weights.

C. Distribution of Hours Spent Caregiving for Household Adults among Non-Elderly Caregivers



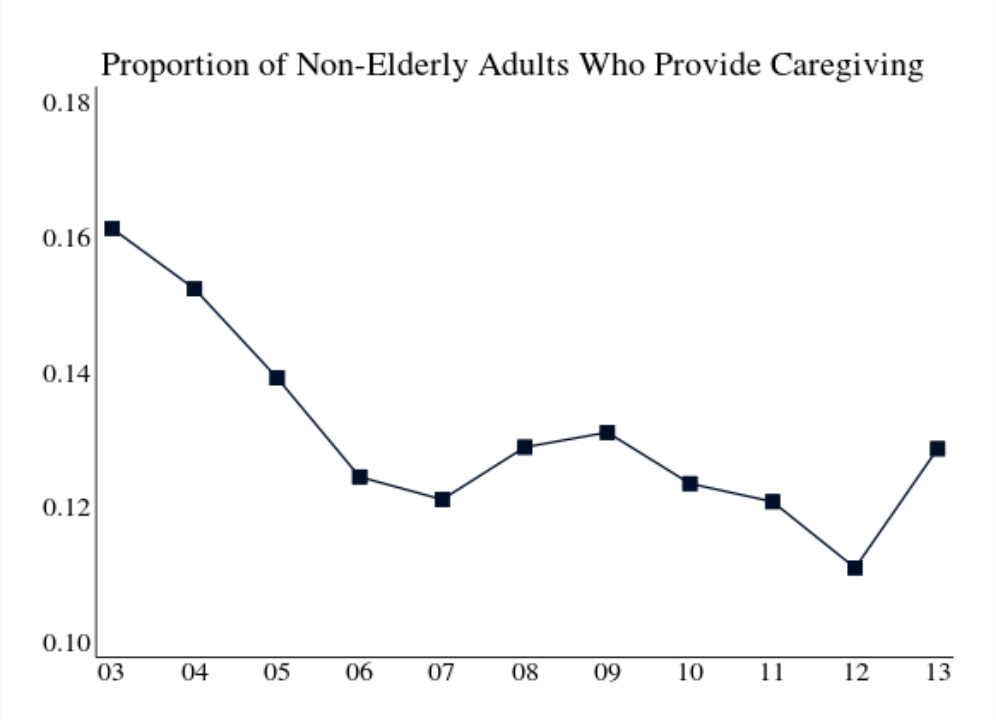
Source: Author's calculations based on ATUS 2003-13. Sample is restricted to adults aged 27-64 (N=101,423). For presentation purposes, those who spend more than 50 hours per week caregiving are omitted from this figure. Estimates include ATUS sampling weights.

D. Distribution of Hours Spent Caregiving for Non-Household Adults among Non-Elderly Caregivers



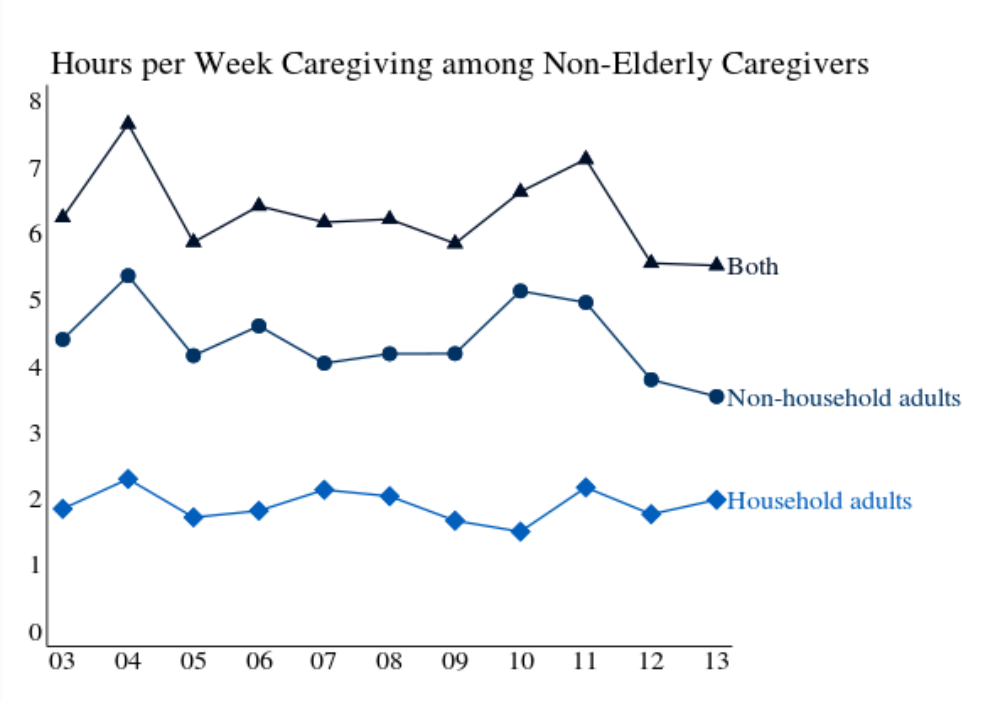
Source: Author's calculations based on ATUS 2003-13. Sample is restricted to adults aged 27-64 (N=101,423). For presentation purposes, those who spend more than 50 hours per week caregiving are omitted from this figure. Estimates include ATUS sampling weights.

E. Trends in Proportion of Nonelderly Adults who Provide Caregiving



Source: Author's calculations based on ATUS 2003-13. Sample is restricted to adults aged 27-64 (N=101,423). Estimates include ATUS sampling weights.

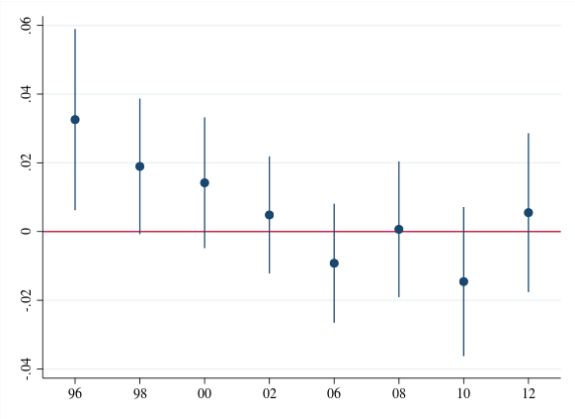
F. Trends in Caregiving Hours Per week among non-Elderly Caregivers



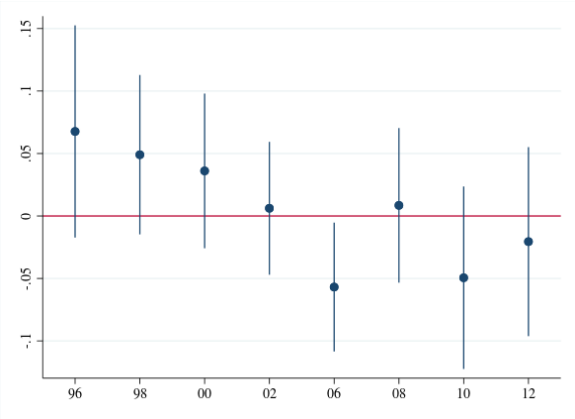
Source: Author's calculations based on ATUS 2003-13. Sample is restricted to adults aged 27-64 who spend greater than 0 hours per week caregiving for household or non-household adults (N=12,876). Estimates include ATUS sampling weights.

Appendix Figure 2- 3. Event Study Results for ADL & IADL Limitations

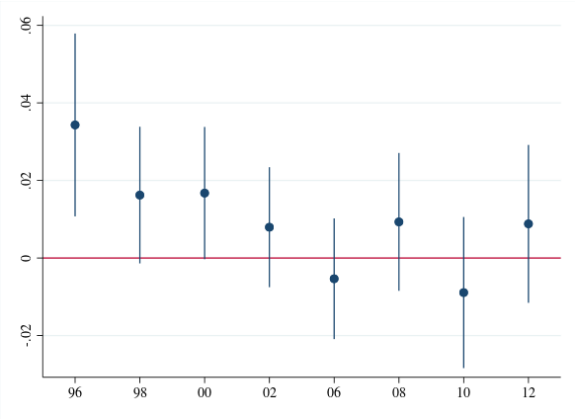
A. Any ADL or IADL limitation



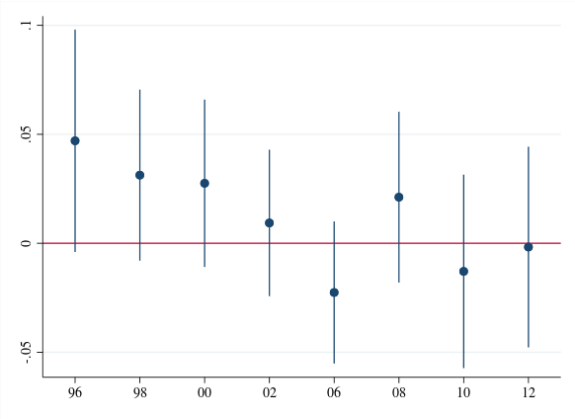
D. Number of ADL & IADL limitations



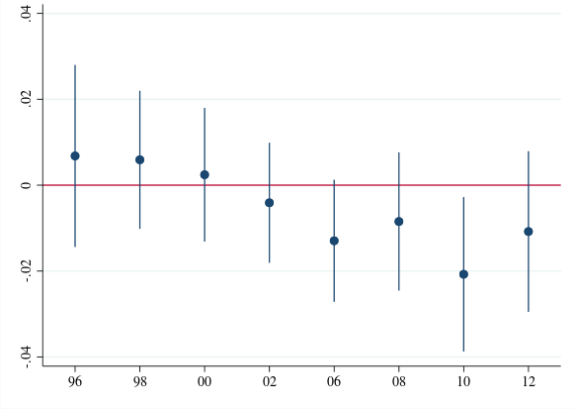
B. Any ADL limitation



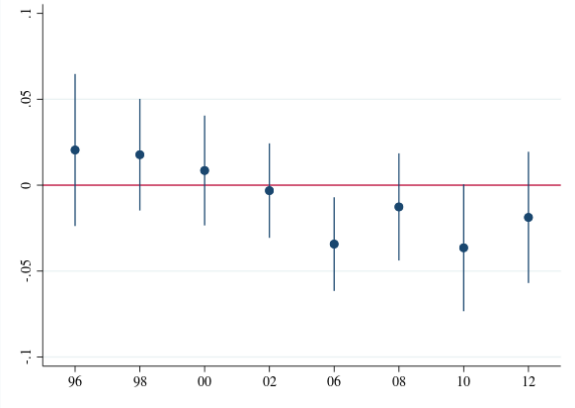
E. Number of ADL limitations



C. Any IADL limitation



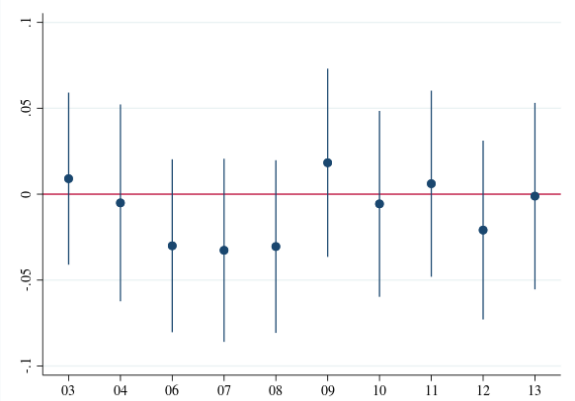
F. Number of IADL limitations



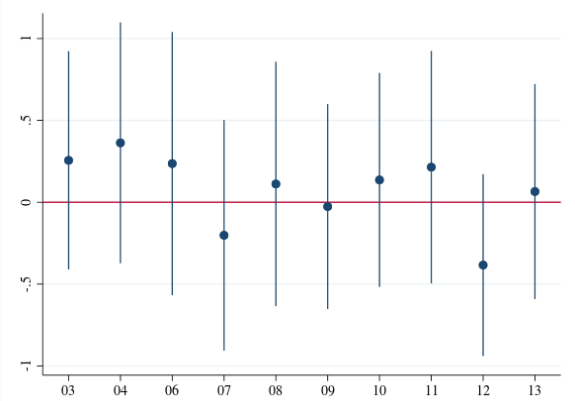
Source: Author's calculations based on Health and Retirement Study, 1996-2012. Figure displays coefficient and 95% confidence intervals for the interaction of treatment and each year indicator. All regressions also control for respondent's household income, sex, race/ethnicity, educational attainment, region of residence, and marital status; age fixed effects; and year fixed effects. Estimates include HRS sampling weights, and standard errors are clustered at the individual level.

Appendix Figure 2- 4. Event Study Results for Non-Elderly Adults' Time Spent Caregiving

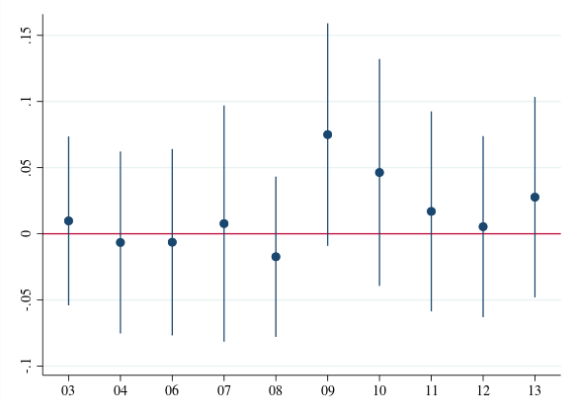
A. Any caregiving (Pooled)



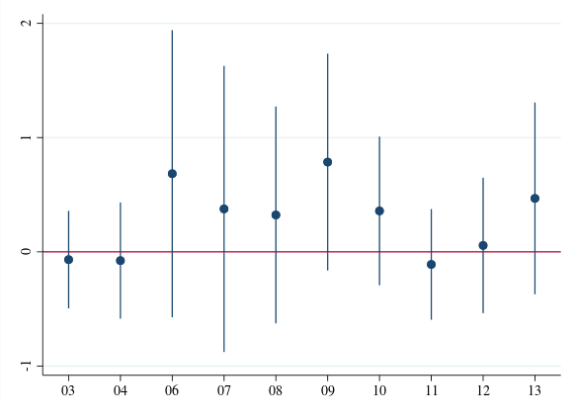
D. Hours caregiving (Pooled)



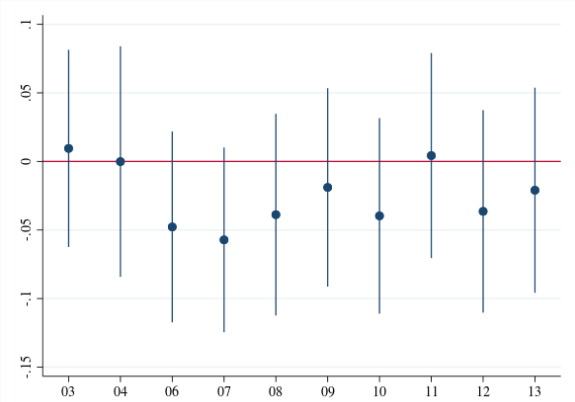
B. Any caregiving (Men)



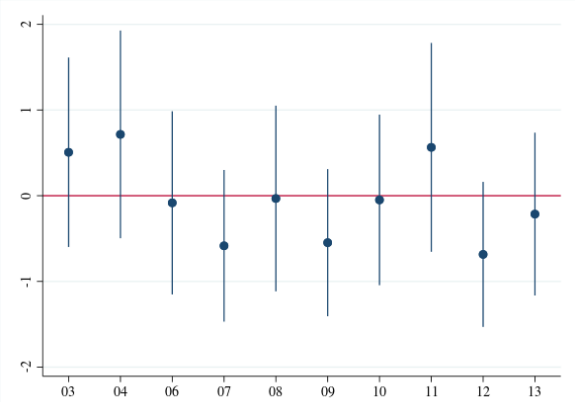
E. Hours caregiving (Men)



C. Any caregiving (Women)



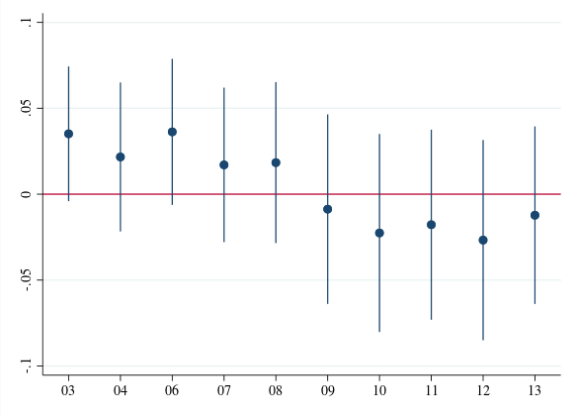
F. Hours caregiving (Women)



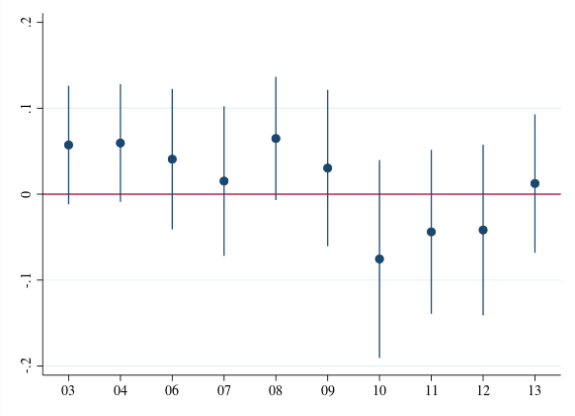
Source: Author's calculations based on American Time Use Survey, 2003-2009. Figure displays coefficient and 95% confidence intervals for the interaction of treatment and each year indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights, and standard errors are robust.

Appendix Figure 2- 5. Event Study Results for Non-Elderly Adults' Labor Force Outcomes

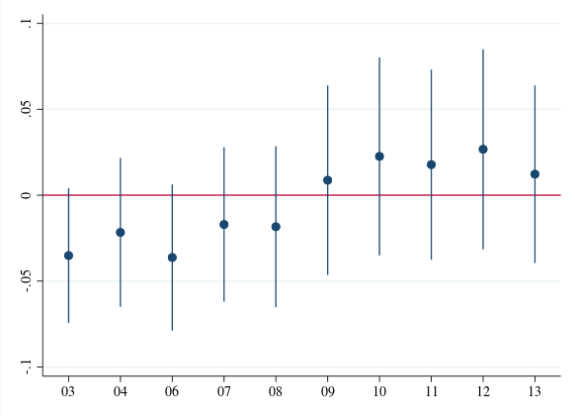
A. Employed (Pooled)



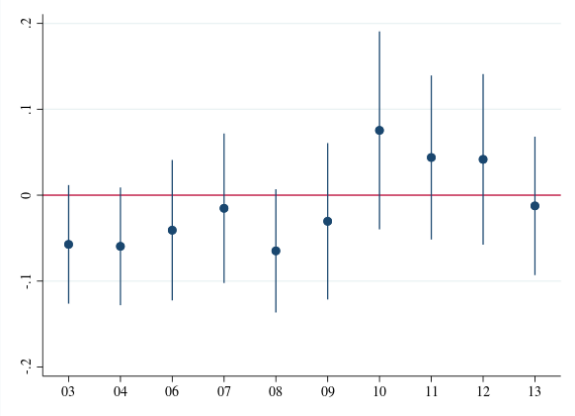
D. Employed (Men)



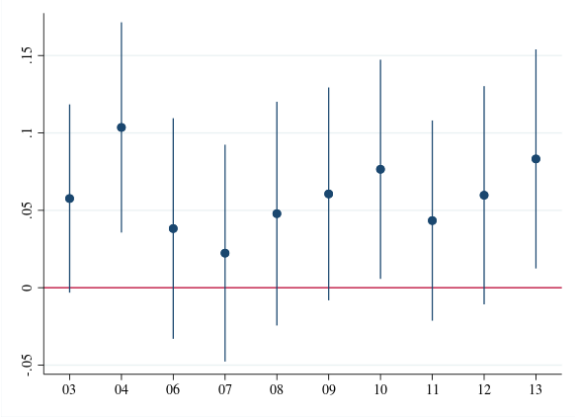
B. Unemployed (Pooled)



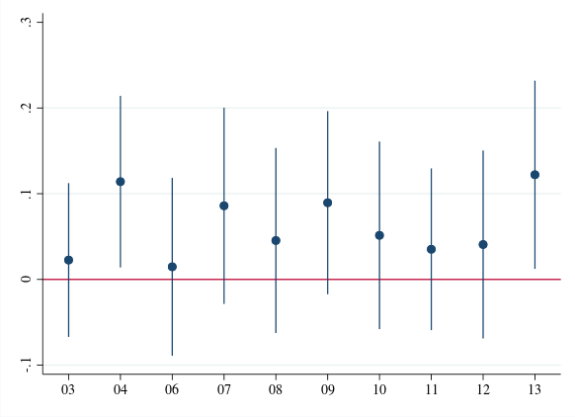
E. Unemployed (Men)



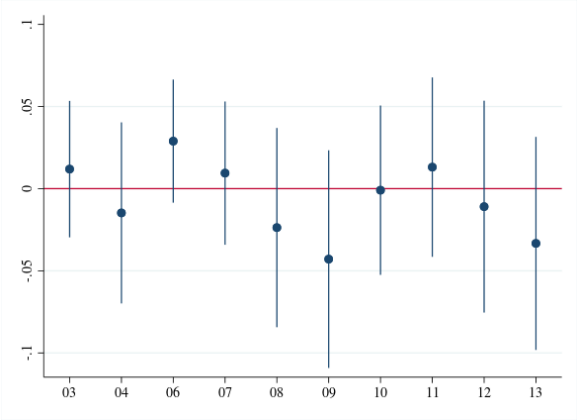
C. Not in labor force (Pooled)



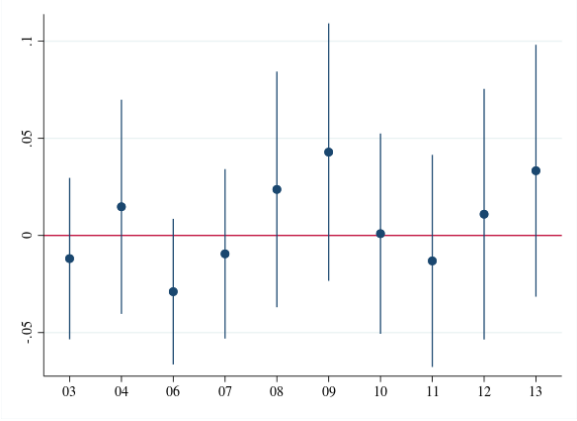
F. Not in labor force (Men)



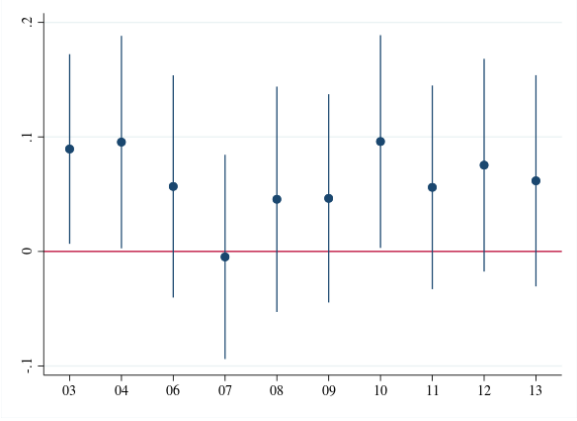
G. Employed (Women)



H. Unemployed (Women)



I. Not in labor force (Women)



Source: Author's calculations based on American Time Use Survey, 2003-2009. Figure displays coefficient and 95% confidence intervals for the interaction of treatment and each year indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights, and standard errors are robust.

2-C. Appendix 2 Tables

Appendix Table 2- 2. Demographic Characteristics of HRS Sample

	Control group	Treatment group
Age	59.16	69.29
Male	0.477	0.459
Family income	82358.4	59697.6
Less than high school	0.163	0.241
High school	0.311	0.350
Some college	0.242	0.196
College or more	0.281	0.213
White, non-Hispanic	0.784	0.817
Black, non-Hispanic	0.102	0.0907
Other, non-Hispanic	0.0307	0.0220
Hispanic	0.0828	0.0703
Married	0.692	0.653
Northeast	0.172	0.178
Midwest	0.246	0.246
South	0.384	0.376
West	0.197	0.198
Observations	56,520	49,765

Source: Author's calculations based on HRS 1996-2012. Estimates include HRS sampling weights.

Appendix Table 2- 3. Demographic Characteristics of ATUS Sample

	Control group	Treatment group
Age	44.40	50.24
Male	0.498	0.407
Family income	75457.7	66309.6
Less than high school	0.103	0.122
High school	0.298	0.379
Some college	0.258	0.256
College or more	0.341	0.243
White, non-Hispanic	0.697	0.647
Black, non-Hispanic	0.113	0.147
Other, non-Hispanic	0.0539	0.0743
Hispanic	0.136	0.132
Married	0.673	0.551
Number of people in household	3.042	3.246
Northeast	0.179	0.192
Midwest	0.246	0.201
South	0.353	0.375
West	0.221	0.231
Weekend or holiday	0.299	0.297
Observations	96,422	5,001

Source: Author's calculations based on ATUS 2003-2013. Estimates include ATUS sampling weights.

Appendix Table 2- 4. DD Estimates for Impact of Medicare Part D on Specific Limitations

	Pooled	Married	Not married	Men	Women
Dressing	-0.004	-0.005	-0.002	-0.004	-0.003
	(0.005)	(0.005)	(0.009)	(0.007)	(0.006)
	[μ=0.082]	[μ=0.073]	[μ=0.099]	[μ=0.076]	[μ=0.086]
Bathing/showering	-0.010***	-0.009**	-0.011	-0.011**	-0.009*
	(0.004)	(0.004)	(0.008)	(0.005)	(0.005)
	[μ=0.053]	[μ=0.043]	[μ=0.072]	[μ=0.042]	[μ=0.063]
Walking across room	-0.000	-0.004	0.009	-0.004	0.002
	(0.004)	(0.004)	(0.008)	(0.005)	(0.006)
	[μ=0.050]	[μ=0.041]	[μ=0.067]	[μ=0.043]	[μ=0.056]
Getting in and out of bed	-0.005	-0.005	-0.004	-0.010*	-0.001
	(0.004)	(0.004)	(0.008)	(0.005)	(0.005)
	[μ=0.048]	[μ=0.038]	[μ=0.064]	[μ=0.040]	[μ=0.053]
Eating	-0.005**	-0.006**	-0.002	-0.008**	-0.003
	(0.002)	(0.002)	(0.005)	(0.003)	(0.003)
	[μ=0.022]	[μ=0.019]	[μ=0.027]	[μ=0.021]	[μ=0.022]
Shopping for groceries	-0.009**	-0.012***	-0.003	-0.009	-0.010
	(0.004)	(0.005)	(0.009)	(0.006)	(0.006)
	[μ=0.068]	[μ=0.056]	[μ=0.092]	[μ=0.051]	[μ=0.082]
Managing money	-0.010***	-0.011***	-0.007	-0.012***	-0.008**
	(0.003)	(0.003)	(0.006)	(0.005)	(0.004)
	[μ=0.039]	[μ=0.031]	[μ=0.052]	[μ=0.043]	[μ=0.035]
Preparing hot meals	-0.003	-0.006*	0.004	-0.006	-0.001
	(0.003)	(0.004)	(0.007)	(0.005)	(0.005)
	[μ=0.045]	[μ=0.039]	[μ=0.055]	[μ=0.041]	[μ=0.047]
Using a telephone	-0.005*	-0.004	-0.005	-0.004	-0.006**
	(0.003)	(0.003)	(0.005)	(0.004)	(0.003)
	[μ=0.028]	[μ=0.028]	[μ=0.030]	[μ=0.037]	[μ=0.021]
Taking medications	-0.006***	-0.008***	-0.004	-0.006	-0.007**
	(0.002)	(0.003)	(0.005)	(0.004)	(0.003)
	[μ=0.023]	[μ=0.022]	[μ=0.027]	[μ=0.025]	[μ=0.022]
Observations	103,755	70,190	33,565	45,659	58,096

Source: Author's calculations based on Health and Retirement Study, 1996-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for respondent's household income, sex, race/ethnicity, educational attainment, region of residence, and marital status; age fixed effects; and year fixed effects. Estimates include HRS sampling weights. Individual-clustered standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 5. DD Estimates for Impact of Medicare Part D on Other HRS Outcomes

	Pooled	Married	Not married	Men	Women
Any gross motor limitation	-0.002 (0.007) [μ=0.217]	-0.006 (0.008) [μ=0.184]	0.004 (0.014) [μ=0.278]	-0.007 (0.010) [μ=0.171]	0.001 (0.010) [μ=0.255]
Number of gross motor limitations	-0.012 (0.018) [μ=0.426]	-0.027 (0.019) [μ=0.357]	0.014 (0.037) [μ=0.560]	-0.032 (0.024) [μ=0.344]	0.001 (0.026) [μ=0.495]
Any fine motor limitation	0.000 (0.006) [μ=0.127]	-0.004 (0.006) [μ=0.113]	0.007 (0.011) [μ=0.153]	-0.001 (0.008) [μ=0.114]	0.001 (0.008) [μ=0.137]
Number of fine motor limitations	-0.007 (0.008) [μ=0.165]	-0.010 (0.009) [μ=0.147]	0.001 (0.017) [μ=0.200]	-0.013 (0.012) [μ=0.151]	-0.002 (0.011) [μ=0.177]
Very good/excellent self-assessed health	0.025*** (0.009) [μ=0.405]	0.024** (0.011) [μ=0.434]	0.028* (0.016) [μ=0.351]	0.026* (0.014) [μ=0.403]	0.024** (0.012) [μ=0.407]
Regularly use prescription drugs	0.031*** (0.008) [μ=0.806]	0.036*** (0.009) [μ=0.812]	0.023 (0.014) [μ=0.795]	0.021 (0.013) [μ=0.769]	0.041*** (0.010) [μ=0.837]
Any doctor visit	0.010** (0.004) [μ=0.945]	0.013*** (0.005) [μ=0.950]	0.003 (0.009) [μ=0.937]	0.012* (0.007) [μ=0.937]	0.008 (0.005) [μ=0.952]
Number of doctor visits	0.454 (0.337) [μ=10.00]	0.321 (0.398) [μ=9.71]	0.790 (0.613) [μ=10.58]	0.391 (0.424) [μ=9.67]	0.519 (0.513) [μ=10.27]
Any home health care visit	0.001 (0.004) [μ=0.069]	-0.003 (0.004) [μ=0.060]	0.009 (0.007) [μ=0.086]	-0.003 (0.005) [μ=0.063]	0.005 (0.005) [μ=0.074]
Observations	103,755	70,190	33,565	45,659	58,096

Source: Author's calculations based on Health and Retirement Study, 1996-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for respondent's household income, sex, race/ethnicity, educational attainment, region of residence, and marital status; age fixed effects; and year fixed effects. Estimates include HRS sampling weights. Individual-clustered standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 6. Robustness Checks Using Different Age Groups for HRS Outcomes

	55-74	56-73	57-72	58-71	59-70	60-69
Any ADL/IADL limitations	-0.016**	-0.017**	-0.016**	-0.017**	-0.017**	-0.020**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Number ADL/IADL limitations [0-10]	-0.056**	-0.058**	-0.056**	-0.059**	-0.056**	-0.052*
	(0.023)	(0.024)	(0.025)	(0.026)	(0.027)	(0.027)
Any ADL Limitations	-0.011*	-0.012*	-0.010	-0.011	-0.011	-0.012
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Number ADL limitations [0-5]	-0.023*	-0.025*	-0.023	-0.022	-0.019	-0.020
	(0.014)	(0.014)	(0.015)	(0.016)	(0.017)	(0.017)
Any IADL Limitations	-0.015***	-0.016***	-0.015***	-0.017***	-0.017**	-0.017**
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Number IADL limitations [0-5]	-0.032***	-0.033***	-0.033***	-0.037***	-0.037***	-0.031**
	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)
Any gross motor limitation	-0.002	-0.005	-0.005	-0.004	-0.002	-0.003
	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
Number of gross motor limitations	-0.012	-0.019	-0.019	-0.020	-0.015	-0.022
	(0.018)	(0.019)	(0.020)	(0.021)	(0.022)	(0.022)
Any fine motor limitation	0.000	-0.001	-0.001	-0.000	0.001	-0.002
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Number of fine motor limitations	-0.007	-0.008	-0.007	-0.006	-0.005	-0.009
	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Very good/excellent self-assessed health	0.025***	0.026***	0.021**	0.018*	0.018	0.016
	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)
Regularly use prescription drugs	0.031***	0.032***	0.032***	0.024***	0.023**	0.013
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)
Any doctor visit	0.010**	0.010**	0.009*	0.008	0.009	0.010*
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Number of doctor visits	0.454	0.312	0.277	0.305	0.281	-0.035
	(0.337)	(0.356)	(0.391)	(0.391)	(0.406)	(0.442)
Any home health care visit	0.001	0.001	0.000	-0.001	-0.001	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	103,755	93,664	83,400	72,861	62,791	52,513

Source: Author's calculations based on Health and Retirement Study, 1996-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for respondent's household income, sex, race/ethnicity, educational attainment, region of residence, and marital status; age fixed effects; and year fixed effects. Estimates include HRS sampling weights. Individual-clustered standard errors are displayed in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 7. Sensitivity Analyses for HRS Outcomes

	Control for Medicare Advantage penetration	Non-clustered errors	Exclude ages 63-64	Exclude year 2004	Excludes years 2010+	No controls	Individual FE
Any ADL/IADL limitations	-0.016**	-0.016***	-0.014**	-0.020***	-0.015**	-0.014**	-0.010
	(0.007)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Number ADL/IADL limitations [0-10]	-0.056**	-0.056***	-0.054**	-0.065***	-0.048**	-0.053**	-0.023
	(0.023)	(0.018)	(0.025)	(0.025)	(0.023)	(0.023)	(0.017)
Any ADL Limitations	-0.011*	-0.011**	-0.010	-0.015**	-0.010	-0.010	-0.008
	(0.006)	(0.005)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Number ADL limitations [0-5]	-0.024*	-0.023**	-0.022	-0.030**	-0.019	-0.022	-0.004
	(0.014)	(0.011)	(0.015)	(0.015)	(0.014)	(0.014)	(0.011)
Any IADL Limitations	-0.015***	-0.015***	-0.014**	-0.016***	-0.011**	-0.014**	-0.010*
	(0.005)	(0.004)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
Number IADL limitations [0-5]	-0.032***	-0.032***	-0.032***	-0.035***	-0.029***	-0.031***	-0.019*
	(0.011)	(0.009)	(0.012)	(0.012)	(0.011)	(0.011)	(0.010)
Any gross motor limitation	-0.002	-0.002	-0.000	-0.002	-0.009	0.002	-0.012*
	(0.007)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
Number of gross motor limitations	-0.012	-0.012	-0.008	-0.017	-0.020	-0.007	-0.023
	(0.018)	(0.014)	(0.020)	(0.019)	(0.018)	(0.019)	(0.014)
Any fine motor limitation	0.000	0.000	-0.002	-0.002	0.004	0.002	0.007
	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Number of fine motor limitations	-0.007	-0.007	-0.008	-0.012	-0.003	-0.005	0.004
	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Very good/excellent self-assessed health	0.025***	0.025***	0.028***	0.025**	0.018*	0.018*	0.014*
	(0.009)	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)	(0.008)
Regularly use prescription drugs	0.031***	0.031***	0.033***	0.030***	0.025***	0.031***	0.010
	(0.008)	(0.006)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)
Any doctor visit	0.011**	0.010***	0.012**	0.010**	0.005	0.008*	0.002
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
Number of doctor visits	0.477	0.454	0.546	0.475	0.601*	0.394	-0.373
	(0.338)	(0.298)	(0.362)	(0.354)	(0.352)	(0.333)	(0.399)
Any home health care visit	0.001	0.001	0.002	0.000	-0.002	0.001	0.002
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Observations	103,578	103,755	92,560	92,233	80,031	103,902	98,688

Source: Author's calculations based on Health and Retirement Study, 1996-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. Unless otherwise indicated, all regressions also control for respondent's household income, sex, race/ethnicity, educational attainment, region of residence, and marital status; age fixed effects; and year fixed effects. Estimates include HRS sampling weights. Unless otherwise indicated, individual-clustered standard errors are displayed in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 8. DD Estimates for Impact of Household's Part D Eligibility on Non-Elderly Adults' Time Spent Caregiving (Detailed Activities)

	Pooled	Men	Women
Physical care for household adults	-0.026 (0.118) [μ=0.412]	0.173 (0.109) [μ=0.117]	-0.174 (0.189) [μ=0.630]
Looking after household adults (as a primary activity)	0.004 (0.021) [μ=0.025]	-0.009 (0.010) [μ=0.014]	0.013 (0.035) [μ=0.034]
Providing medical care to household adults	-0.059 (0.052) [μ=0.118]	0.016 (0.021) [μ=0.029]	-0.117 (0.090) [μ=0.183]
Obtaining medical and care services for household adults	-0.018 (0.049) [μ=0.094]	0.070 (0.056) [μ=0.005]	-0.083 (0.074) [μ=0.159]
Waiting associated with caring for household adults	-0.036 (0.049) [μ=0.098]	0.034 (0.075) [μ=0.071]	-0.085 (0.065) [μ=0.117]
Caring for household adults, n.e.c.	-0.027 (0.033) [μ=0.038]	0.002 (0.005) [μ=0.004]	-0.048 (0.056) [μ=0.063]
Helping household adults	-0.013 (0.027) [μ=0.061]	0.030 (0.028) [μ=0.005]	-0.041 (0.043) [μ=0.103]
Organization and planning for household adults	-0.016 (0.015) [μ=0.031]	0.006 (0.005) [μ=0.002]	-0.031 (0.025) [μ=0.052]
Picking up or dropping off household adults	-0.029* (0.016) [μ=0.055]	-0.034 (0.023) [μ=0.057]	-0.025 (0.023) [μ=0.053]
Waiting associated with helping household adults	0.028*** (0.011) [μ=0.014]	0.042*** (0.013) [μ=0.006]	0.017 (0.016) [μ=0.020]
Helping household adults, n.e.c.	0.006 (0.020) [μ=0.018]	0.052* (0.030) [μ=<0.001]	-0.027 (0.027) [μ=0.032]
Observations	101,423	45,193	56,230

Source: Author's calculations based on American Time Use Survey, 2003-2013. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights. Robust standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 9. Heterogeneity Tests for Part D's Impact on Non-Elderly Adults' Time Use

	Men Caregiving (Any)	Men Caregiving (hours per week)	Observations	Women Caregiving (Any)	Women Caregiving (hours per week)	Observations
In labor force	0.017 (0.020) [μ=0.082]	0.451** (0.189) [μ=0.233]	39,715	-0.063*** (0.024) [μ=0.172]	-0.807*** (0.282) [μ=1.320]	41,958
Not in labor force	0.011 (0.036) [μ=0.088]	0.067 (0.361) [μ=0.592]	5,478	0.014 (0.028) [μ=0.144]	-0.196 (0.531) [μ=1.655]	14,272
No children in household	0.033* (0.020) [μ=0.074]	0.446** (0.193) [μ=0.303]	22,497	-0.040* (0.020) [μ=0.164]	-0.623* (0.323) [μ=1.503]	24,552
Have children	-0.040 (0.039) [μ=0.119]	0.148 (0.322) [μ=0.342]	22,696	-0.018 (0.040) [μ=0.154]	-0.598 (0.403) [μ=1.260]	31,678
Age 27-44	0.023 (0.027) [μ=0.070]	0.302 (0.251) [μ=0.303]	22,785	-0.062 (0.042) [μ=0.143]	-0.637 (0.396) [μ=0.927]	29,222
Age 45-54	0.025 (0.032) [μ=0.094]	0.531 (0.332) [μ=0.339]	12,785	-0.010 (0.040) [μ=0.200]	-0.347 (0.502) [μ=1.624]	14,882
Age 55-64	-0.004 (0.033) [μ=0.096]	0.367 (0.268) [μ=0.289]	9,623	-0.034 (0.023) [μ=0.151]	-0.780* (0.417) [μ=1.589]	12,126
White, non-Hispanic	0.018 (0.023) [μ=0.093]	0.275 (0.211) [μ=0.361]	31,828	-0.048** (0.023) [μ=0.179]	-0.675** (0.331) [μ=1.451]	37,795
Black, non-Hispanic	0.041 (0.033) [μ=0.049]	0.315 (0.335) [μ=0.369]	5,031	-0.005 (0.040) [μ=0.143]	-0.336 (0.588) [μ=1.439]	8,031
Other, non-Hispanic	-0.033 (0.066) [μ=0.088]	1.301* (0.774) [μ=0.079]	2,371	-0.104 (0.080) [μ=0.227]	-2.790 (1.789) [μ=3.768]	2,971
Hispanic	0.002 (0.045) [μ=0.082]	0.494 (0.425) [μ=0.148]	5,963	0.055* (0.028) [μ=0.047]	0.399 (0.325) [μ=0.491]	7,433
Less than high school	-0.029 (0.053) [μ=0.124]	0.272 (0.358) [μ=0.324]	4,232	-0.007 (0.067) [μ=0.129]	0.159 (0.626) [μ=1.060]	5,071
High school	0.055** (0.025)	0.491* (0.269)	12,183	-0.067** (0.030)	-0.905*** (0.326)	14,221

	[μ=0.057]	[μ=0.255]		[μ=0.178]	[μ=1.437]	
Some college	-0.001 (0.036) [μ=0.093]	0.538 (0.355) [μ=0.276]	12,106	-0.017 (0.034) [μ=0.172]	-0.994 (0.735) [μ=2.078]	16,672
College or more	0.004 (0.039) [μ=0.094]	0.089 (0.330) [μ=0.439]	16,672	-0.013 (0.030) [μ=0.134]	-0.034 (0.395) [μ=0.851]	20,266
Married	-0.022 (0.030) [μ=0.057]	0.445* (0.241) [μ=0.338]	28,882	-0.038* (0.022) [μ=0.178]	-0.556* (0.318) [μ=1.726]	32,906
Not married	0.036* (0.021) [μ=0.124]	0.316 (0.220) [μ=0.269]	16,311	-0.026 (0.031) [μ=0.153]	-0.673 (0.455) [μ=1.286]	23,324

Source: Author's calculations based on American Time Use Survey, 2003-2013. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights. Robust standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 10. DD Estimates for Impact of Household's Part D Eligibility on Non-Elderly Adults' Labor Force Outcomes (Employed Adults Only)

	Pooled	Men	Women
Full-time employed	-0.006	-0.026	0.003
	(0.018)	(0.022)	(0.029)
	[μ=0.812]	[μ=0.914]	[μ=0.719]
Part-time employed	0.006	0.026	-0.003
	(0.018)	(0.022)	(0.029)
	[μ=0.188]	[μ=0.086]	[μ=0.281]
Self-employed	0.012	0.013	0.008
	(0.015)	(0.025)	(0.018)
	[μ=0.109]	[μ=0.124]	[μ=0.095]
Wage earner	-0.013	-0.014	-0.008
	(0.015)	(0.025)	(0.018)
	[μ=0.890]	[μ=0.876]	[μ=0.903]
Work without pay	0.001	0.001	0.000
	(0.001)	(0.001)	(0.002)
	[μ<0.001]	[μ=<0.001]	[μ=0.002]
Usual hours worked	-0.013	0.897	-1.315
	(0.654)	(0.973)	(0.891)
	[μ=40.71]	[μ=43.33]	[μ=38.35]
Observations	77,174	37,753	39,421

Source: Author's calculations based on American Time Use Survey, 2003-2013. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights. Robust standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 11. Sensitivity Analyses for ATUS Outcomes

	Control for Medicare Advantage penetration		
	Pooled	Men	Women
Caregiving (any)	-0.013	0.017	-0.035*
	(0.013)	(0.017)	(0.018)
Caregiving (hours per week)	-0.186	0.382**	-0.602**
	(0.166)	(0.165)	(0.261)
Observations	101,423	45,193	56,230

Source: Author's calculations based on American Time Use Survey, 2003-2013. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. Unless otherwise indicated, all regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights. Robust standard errors are displayed in parentheses.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table 2- 12. DD Estimates for Impact of Household's Part D Eligibility on Time Use of Non-Elderly Women Aged 55-64

	DD estimate	Pre-2006 mean
Sleeping	0.062 (0.887)	58.49
Grooming	-0.204 (0.278)	5.526
Health-Related Self Care	-0.034 (0.543)	0.876
Personal Activities	-0.045 (0.052)	0.0953
Personal Care Emergencies	-0.000 (0.000)	0
Housework	1.826*** (0.670)	6.324
Food and Drink Preparation, Presentation, and Clean-up	0.498 (0.481)	6.797
Interior Maintenance, Repair, and Decoration	-0.089 (0.359)	0.933
Exterior Maintenance, Repair, and Decoration	0.155 (0.158)	0.258
Lawn, Garden, and Houseplants	0.068 (0.439)	1.723
Animals and Pets	0.010 (0.196)	0.800
Vehicles	-0.023 (0.050)	0.0970
Appliances, Tools, and Toys	0.008 (0.032)	0.0126
Household Management	0.070 (0.320)	1.931
Caring for and Helping Household Children	-0.043 (0.162)	0.398
Activities Related to Household Children's Education	-0.045 (0.041)	0.0652
Activities Related to Household Children's Health	0.001 (0.013)	0.00456
Caring for Household Adults	-0.660 (0.407)	1.384
Helping Household Adults	-0.120* (0.073)	0.205
Caring for and Helping Non-Household Children	1.111*** (0.392)	1.041
Activities Related to Non-Household Children's Education	0.045* (0.023)	0.00328
Activities Related to Non-Household Children's Health	0.014 (0.032)	0.0216
Caring for Non-Household Adults	-0.534** (0.265)	0.666

Helping Non-Household Adults	0.004 (0.185)	0.557
Working	-1.738 (1.768)	16.71
Work-Related Activities	0.020 (0.040)	0.0386
Other Income-Generating Activities	-0.019 (0.167)	0.225
Job Search and Interviewing	0.015 (0.113)	0
Taking Class	0.017 (0.127)	0.203
Extracurricular School Activities (except sports)	0.000 (.)	0
Research or Homework	0.195* (0.106)	0.0713
Registration or Administrative Activities	0.001 (0.002)	0
Shopping (store, telephone, internet)	-0.346 (0.469)	4.158
Researching Purchases	0.015 (0.010)	0
Security Procedures Related to Consumer Purchases	0.000 (.)	0
Childcare Services	-0.001* (0.001)	0
Financial Services and Banking	-0.039 (0.043)	0.0695
Legal Services	-0.005 (0.003)	0.00250
Medical and Care Services	-0.432 (0.273)	0.840
Personal Care Services	-0.097 (0.103)	0.243
Real Estate	0.019 (0.025)	0.0117
Veterinary Services (excluding grooming)	0.045 (0.031)	0.00421
Security Procedures Related to Professional or Personal Services	0.000 (.)	0
Household Services (not done by self)	0.027** (0.012)	0.00304
Home Maintenance, Repair, Decoration, and Construction (not done by self)	0.136 (0.088)	0.0406
Pet Services (not done by self and not veterinary care)	-0.035 (0.032)	0.0316
Lawn and Garden Services (not done by self)	0.001 (0.001)	0

Vehicle Maintenance and Repair Services (not done by self)	0.037 (0.033)	0.00756
Using Government Services	0.041 (0.030)	0
Civic Obligations and Participation	-0.005 (0.072)	0.0639
Waiting Associated with Government Services or Civic Obligations	-0.000 (0.009)	0.00170
Security Procedures Related to Government Services or Civic Obligations	0.000 (.)	0
Eating and Drinking	0.331 (0.362)	8.343
Waiting Associated with Eating and Drinking	0.014 (0.009)	0.00702
Socializing and Communicating	0.250 (0.547)	4.341
Attending or Hosting Social Events	-0.076 (0.248)	0.712
Relaxing and Leisure	0.196 (1.373)	28.37
Arts and Entertainment (other than sports)	-0.175 (0.251)	0.817
Waiting Associated with Socializing, Relaxing, and Leisure	-0.002 (0.010)	0.00910
Participating in Sports, Exercise, or Recreation	-0.153 (0.250)	1.234
Attending Sports or Recreational Events	0.047 (0.062)	0.0528
Waiting Associated with Sports, Exercise, and Recreation	-0.000 (0.008)	0.00668
Security Procedures Related to Sports, Exercise, and Recreation	0.000 (0.000)	0
Religious or Spiritual Practices	0.261 (0.261)	1.208
Administrative and Support Activities	0.197 (0.139)	0.280
Social Service and Care Activities (except medical)	0.134 (0.119)	0.185
Indoor and Outdoor Maintenance, Building, and Clean-Up Activities	0.041 (0.069)	0.0399
Participating in Performance and Cultural Activities	0.098** (0.046)	0.00990
Attending Meetings, Conferences, and Training	0.054 (0.099)	0.137
Public Health and Safety Activities	-0.016 (0.014)	0
Waiting Associated with Volunteer Activities	0.005 (0.006)	0

Security Procedures Related to Volunteer Activities	0.000 (.)	0
Telephone Calls (to or from)	0.292* (0.164)	0.811
Waiting Associated with Telephone Calls	0.000 (.)	0
Travel Related to Personal Care	-0.276 (0.214)	0.423
Travel Related to Household Activities	-0.137 (0.097)	0.338
Travel Related to Caring for and Helping Household Members	-0.104 (0.140)	0.322
Travel Related to Caring for and Helping Non-Household Members	-0.140 (0.151)	0.767
Travel Related to Work	-0.235 (0.219)	1.303
Travel Related to Education	-0.020 (0.042)	0.0476
Travel Related to Consumer Purchases	0.012 (0.239)	2.001
Travel Related to Using Professional and Personal Care Services	-0.049 (0.109)	0.417
Travel Related to Using Household Services	0.079 (0.053)	0.0207
Travel Related to Using Government Services and Civic Obligations	0.047* (0.025)	0.00479
Travel Related to Eating and Drinking	-0.029 (0.161)	0.860
Travel Related to Socializing, Relaxing, and Leisure	0.044 (0.229)	1.239
Travel Related to Sports, Exercise, and Recreation	-0.045 (0.059)	0.188
Travel Related to Religious or Spiritual Activities	0.076 (0.074)	0.208
Travel Related to Volunteering	0.110** (0.051)	0.0791
Travel Related to Phone Calls	-0.051* (0.026)	0.0388
Security Procedures Related to Traveling	-0.016 (0.014)	0.0128
Observations	12,126	

Source: Author's calculations based on American Time Use Survey, 2003-2013. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, marital status, and whether the interview was on a weekday or weekend; treatment group indicator; and year fixed effects. Estimates include ATUS sampling weights. Robust standard errors are displayed in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 13. Impact of Part D on Non-Elderly Spouses' Labor Force Outcomes (HRS)

	Pooled	Men	Women
Employed	0.007	-0.022	-0.004
	(0.018)	(0.045)	(0.021)
	[μ=0.386]	[μ=0.494]	[μ=0.370]
Unemployed	0.000	0.015	-0.004
	(0.005)	(0.014)	(0.005)
	[μ=0.013]	[μ=0.019]	[μ=0.013]
Retired	0.023	-0.002	0.016
	(0.017)	(0.045)	(0.019)
	[μ=0.391]	[μ=0.436]	[μ=0.384]
Partly retired	0.008	0.007	0.004
	(0.009)	(0.023)	(0.010)
	[μ=0.073]	[μ=0.082]	[μ=0.072]
Disabled	0.001	-0.002	0.002
	(0.006)	(0.011)	(0.007)
	[μ=0.037]	[μ=0.043]	[μ=0.036]
Not in labor force	-0.032***	0.012	-0.010
	(0.011)	(0.011)	(0.014)
	[μ=0.173]	[μ=0.008]	[μ=0.197]
Observations	34,572	13,485	21,087

Source: Author's calculations based on Health and Retirement Study, 1996-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for respondent's household income, sex, race/ethnicity, educational attainment, region of residence, and marital status; age fixed effects; and year fixed effects. Estimates include HRS sampling weights. Individual-clustered standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2- 14. Impact of Part D on Non-Elderly Adults' Labor Force Outcomes (CPS)

	Pooled	Men	Women
Employed	-0.013***	-0.023***	-0.006***
	(0.001)	(0.001)	(0.001)
	[μ=0.943]	[μ=0.925]	[μ=0.958]
Unemployed	0.013***	0.023***	0.006***
	(0.001)	(0.001)	(0.001)
	[μ=0.057]	[μ=0.075]	[μ=0.042]
Not in labor force	-0.006***	0.012***	-0.017***
	(0.001)	(0.002)	(0.002)
	[μ=0.366]	[μ=0.278]	[μ=0.425]
Full-time	-0.003***	-0.006***	-0.005***
	(0.001)	(0.002)	(0.002)
	[μ=0.838]	[μ=0.898]	[μ=0.789]
Part-time	0.003***	0.006***	0.005***
	(0.001)	(0.002)	(0.002)
	[μ=0.162]	[μ=0.102]	[μ=0.211]
Self-employed	-0.001	0.003*	-0.004***
	(0.001)	(0.002)	(0.001)
	[μ=0.111]	[μ=0.126]	[μ=0.098]
Wage-earner	0.001	-0.002	0.004***
	(0.001)	(0.002)	(0.001)
	[μ=0.888]	[μ=0.872]	[μ=0.900]
Work without pay	-0.000	-0.000***	0.000
	(0.000)	(0.000)	(0.000)
	[μ=0.001]	[μ=0.001]	[μ=0.002]
Usual hours worked	-0.051	-0.018	-0.226***
	(0.037)	(0.053)	(0.051)
	[μ=39.73]	[μ=41.71]	[μ=38.15]
Observations	11,404,699	5,485,994	5,918,705

Source: Author's calculations based on Current Population Survey, 2000-13. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for respondent's age, sex, race/ethnicity, educational attainment, region of residence, household size, and marital status; treatment group indicator; and year fixed effects. Estimates include CPS sampling weights. Robust standard errors are displayed in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendices for Chapter 3

3-A. Review of Literature on Ex-Ante Moral Hazard in the Presence of Health Insurance

Appendix Table 3- 1. Studies that Evaluate the Impact of Health Insurance on Risky Health Behaviors[39]

Paper	Outcomes Studied	Data/Methods	Results
Medicare			
Dave & Kaestner. (2009). Health Insurance and Ex Ante Moral Hazard: Evidence from Medicare. *International Journal of Health Care Finance and Economics*.	Exercise; Smoking; Drinking	Diff-in-diff comparing pre- and post-age 65 changes in outcomes of those who were uninsured and those who were insured before age 65; HRS	Decreased probability of vigorous exercise, increased cigarette use, and increased alcohol consumption among elderly men. No significant change for women.
DePreux. (2011). Anticipatory Ex Ante Moral Hazard and the Effect of Medicare on Prevention. *Health Economics.*	Exercise; Smoking; Drinking	Diff-in-diff comparing pre- and post-age 65 changes in outcomes of those who were uninsured and those who were insured before age 65; Propensity score matching; HRS	Decreased probability of vigorous exercise just before receiving Medicare (anticipatory). No significant change in drinking or smoking.

[39] Preventive care (cancer screenings, cholesterol tests, etc) may be interpreted as beneficial health behavior or as utilization of health care services. However, this literature review does not include studies that assess the impact of health insurance on preventive care.

Marti & Richards. (2016). Smoking Response to Health And Medical Spending Changes and the Role of Insurance. *Health Economics.*	Smoking cessation after adverse health shock	Triple differences interacting health shock with Medicare eligibility; HRS	Before age 65, cardiovascular health shock reduces smoking in the pre-Medicare uninsured group; but Medicare eligibility blunts this incentive.
Medicare Part D			
Asfaw. (2019). The Effect of Prescription Drug Insurance on Health Behavior: Evidence from Medicare Part D. *Health Economics.*	Exercise; Smoking; BMI	Difference in regression discontinuity comparing 65-69 year olds vs. 60-64 year olds; NHIS; MEPS	Decreased moderate exercise at the extensive and intensive margins. Increased probability of being overweight. No change in vigorous exercise, muscle-strengthening exercise, smoking, BMI, probability of healthy weight, and probability of obesity.
ACA Young Adult Mandate			
Barbaresco, Courtemanche, & Qi. (2015). Impacts of the Affordable Care Act Dependent Coverage Provision on Health-Related Outcomes of Young Adults. *Journal of Health Economics.*	Exercise; Smoking; Drinking; BMI; Risky sexual behaviors	Diff-in-diff comparing 23-25 year olds with 27-29 year olds; BRFSS	Decreased BMI/obesity. Increased risky drinking. No change in smoking, any drinking, exercise, and unmarried pregnancy.
Breslau et al. (2017). Did the Dependent Coverage Expansion Increase Risky Substance Use Among Young Adults? *Drug and Alcohol Dependence.*	Smoking; Drinking; Marijuana; Drugs	Diff-in-diff comparing 19-25 year olds with 26-34 year olds; NSDUH	No change in smoking, drinking, marijuana, or illicit drug utilization.
Lee. (2018). Effects of Health Insurance Coverage on Risky Behaviors. *Health Economics.*	Smoking; Drinking	Regression discontinuity at age 26; NHIS	No change in smoking or drinking.
Oney. (2018). The Effect of Health Insurance on Sexual Health: Evidence from the Affordable Care Act's dependent Coverage Mandate. *Social Science & Medicine.*	Risky sexual behaviors	Diff-in-diff comparing 20-24 year olds with 15-19 and 25-29 year olds; CDC administrative data	Increased chlamydia rates for men and women, and increased gonorrhea rates for women (likely reflects better access to STD screening). No change in probability of unprotected sex.
ACA Medicaid Expansion			

Simon, Soni, & Cawley. (2017). The Impact of Health Insurance on Preventive Care and Health Behaviors: Evidence from the First Two Years of the ACA Medicaid Expansions. *Journal of Policy Analysis & Management.*	Exercise; Smoking; Drinking; BMI	Diff-in-diff comparing Medicaid expansion and non-expansion states; BRFSS	No change in exercise, smoking, drinking, or BMI.
Cawley, Soni, & Simon. (2018). Third Year of Survey Data Shows Continuing Benefits of Medicaid Expansions for Low-Income Childless Adults in the U.S. *Journal of General Internal Medicine.*	Exercise; Smoking; Drinking; BMI	Diff-in-diff comparing Medicaid expansion and non-expansion states; BRFSS	No change in exercise, smoking, drinking, or BMI.
Courtemanche et al. (2018). Early Effects of the Affordable Care Act on Health Care Access, Risky Health Behaviors, and Self-Assessed Health. *Southern Economic Journal.*	Smoking; Drinking; BMI	Triple difference models exploiting pre-ACA uninsured rates and Medicaid expansion; BRFSS	No change in smoking, drinking, or BMI.
He, Lopez, & Boehm. (2018). Medicaid Expansion and Food Choices: The Case of Carbonated Soft Drinks. Working paper.	Purchases of carbonated soft drinks	Diff-in-diff comparing Medicaid expansion and non-expansion states; Nielsen	Decreased spending on carbonated soft drinks purchases.
Cotti, Nesson, & Tefft. (2019). Impacts of the ACA Medicaid Expansion on Health Behaviors: Evidence from Household Panel Data. *Health Economics.*	Purchases of alcohol, nicotine-related, snack food, and carbonated beverage products	Diff-in-diff comparing Medicaid expansion and non-expansion states; Nielsen	Reduced cigarette, snuff, beer, and liquor purchases. Increased purchases of smoking cessation products.
Courtemanche. (2019). Effects of the Affordable Care Act on Health Behaviors After 3 Years. *Eastern Economic Journal.*	Exercise; Smoking; Drinking; BMI	Triple difference models exploiting pre-ACA uninsured rates and Medicaid expansion; BRFSS	Increase in probability of risky drinking (driven by private portion of ACA, not Medicaid). No change in smoking, exercise, or BMI.

Medicaid & CHIP

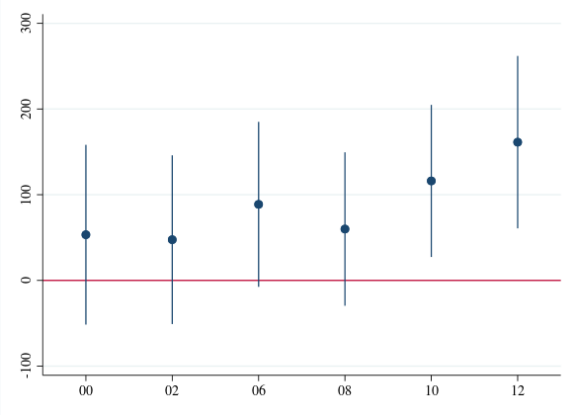
Dave, Kaestner, & Wehby. (2019). Does Public Insurance Coverage for Pregnant Women Affect Prenatal Health Behaviors? *Journal of Population Economics.*	Smoking, BMI, and diet during pregnancy	Exploit exogenous variation in Medicaid income eligibility expansions for pregnant women; Natality files	Increased smoking and pregnancy weight gain.
RAND Health Insurance Experiment			
Brook et al. (1983). Does Free Care Improve Adults' Health? *New England Journal of Medicine.*	Smoking; BMI	Randomized controlled trial; RAND	No impact on smoking or body weight.
Bhattacharya et al. (2011). Does Health Insurance Make You Fat? *Economic Aspects of Obesity.*	BMI	Randomized controlled trial; RAND	Extending insurance coverage to the uninsured increases BMI, but no significant change from increasing the generosity of insurance for the already-covered.
Oregon Health Insurance Experiment			
Baicker et al. (2013). The Oregon Experiment — Effects of Medicaid on Clinical Outcomes. *New England Journal of Medicine*.	Smoking; BMI	Randomized controlled trial; Oregon	No impact on obesity or smoking.
Massachusetts Health Care Reform			
Courtemanche & Zapata. (2014). Does Universal Coverage Improve Health? The Massachusetts Experience. *Journal of Policy Analysis & Management.*	Exercise; Smoking; BMI	Diff-in-diff comparing Massachusetts and other US states; BRFSS	Decreased BMI. No impact on smoking and exercise.
Tennessee Medicaid Disenrollment			
Tello-Trillo. (2016). Effects of Losing Public Health Insurance on Health Care Access, Utilization and Health Outcomes: Evidence from the TennCare Disenrollment. Working Paper.	Exercise; Smoking; Drinking; Diet	Diff-in-diff comparing Tennessee and other Southern states; BRFSS; NHIS	Medicaid disenrollment reduced healthy behaviors for low-educated individuals but not high-educated individuals.

Other settings			
Courbage & de Coulon. (2004). Prevention and Private Health Insurance in the U.K. *Geneva Papers on Risk and Insurance*.	Exercise; Smoking	Instrumental variables exploiting purchase of private insurance in Britain; British Household Panel Survey	Private insurance increased exercise and decreased smoking.
Klick & Stratmann. (2007). Diabetes Treatment and Moral Hazard. *Journal of Law and Economics*.	BMI	Triple differences models exploiting state laws that require plans to cover diabetes treatment; BRFSS	Increased BMI after adoption of mandates.
Kelly & Markowitz. (2009). Incentives in Obesity and Health Insurance. *Inquiry.*	BMI	Instrumental variables using the percent of each state's workforce employed in large firms as an instrument; BRFSS	Having insurance increased BMI but not probability of being obese.
Yilma, Kempen, & de Hoop. (2012). A Perverse 'Net' Effect? Health Insurance and Ex-Ante Moral Hazard in Ghana. *Social Science and Medicine.*	Insecticide-treated bed nets (to protect against malaria)	Household fixed effects model to test impact of enrollment in Ghana's National Health Insurance Scheme; Panel of 400 households in Ghana	Having insurance reduced use of insecticide-treated bed nets
Miller, Pinto, & Vera-Hernández. (2013). Risk Protection, Service Use, and Health Outcomes under Colombia's Health Insurance Program for the Poor. *AEJ: Economic Policy.*	Drinking and smoking during pregnancy	Regression discontinuity exploiting eligibility by income for Colombia's public insurance program; Colombian household survey data	No impact of insurance on drinking and smoking during pregnancy.
Qin & Lu. (2014). Does Health Insurance Lead to Ex ante Moral Hazard? Evidence from China's New Rural Cooperative Medical Scheme. *The Geneva Papers on Risk and Insurance - Issues and Practice.*	Exercise; Smoking; Drinking; Diet; BMI	Instrumental variables approach exploiting implementation of China's New Rural Cooperative Medical Scheme; China Health and Nutrition Survey	Increase in smoking, heavy drinking (among males), sedentary activities, consuming high-calorie food, and being overweight.
Yoruk. (2017). Health Insurance Coverage and Risky Health Behaviors among Young Adults. *BE Journal of Economic Analysis and Policy*.	Smoking; Drinking; Marijuana; Risky sexual behaviors	Regression discontinuity at age 19, when many health insurance contracts stop covering dependents; NLSY	Losing insurance decreased probability of risky drinking. No change in smoking, marijuana, and risky sexual behaviors.
Yu & Zhu. (2018). Affordable Care Encourages Healthy Living: Theory and Evidence from China's New Cooperative Medical Scheme. *Health Economics.*	Smoking	Diff-in-diff and instrumental variables approach exploiting implementation of China's New Rural Cooperative Medical Scheme; China Health and Nutrition Survey	Decreased amount of cigarette consumption. No change in probability of smoking or smoking cessation.

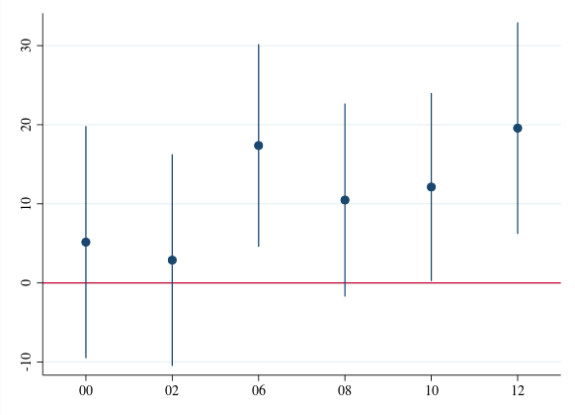
3-B. Appendix 3 Figures

Appendix Figure 3- 1. Event Study Results for NHANES Outcomes

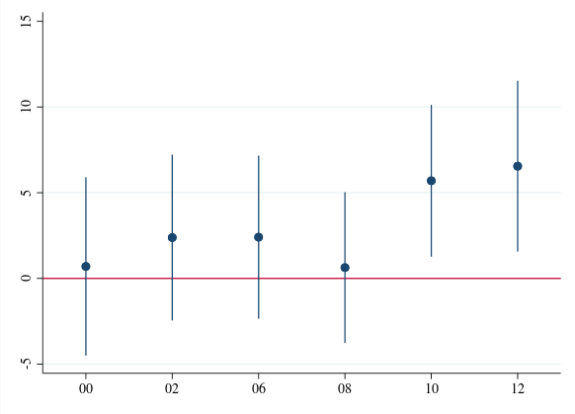
Calories (kcal)



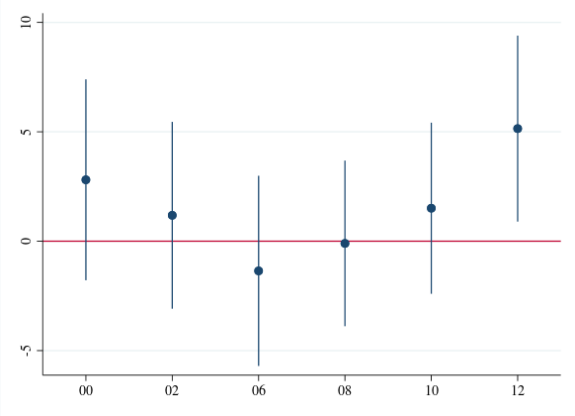
Carbohydrates (gm)



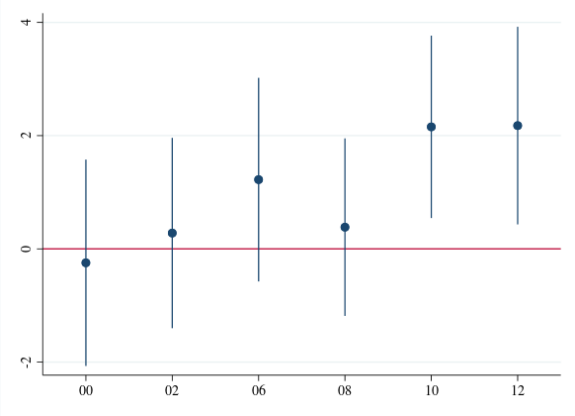
Total fat (gm)



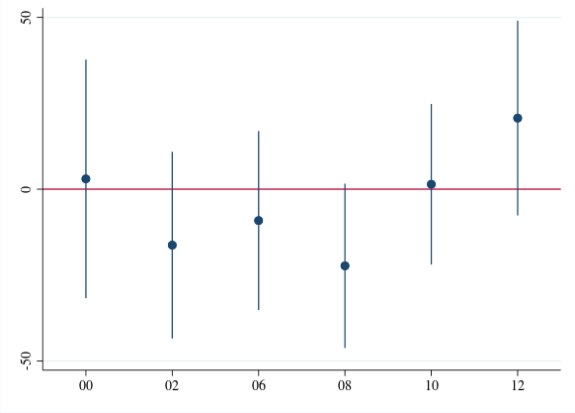
Protein (gm)



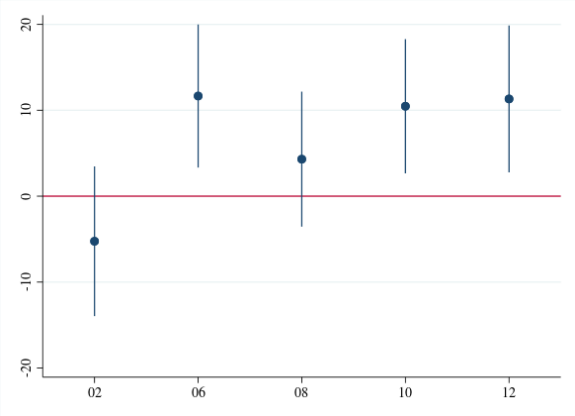
Total saturated fatty acids (gm)



Cholesterol (mg)

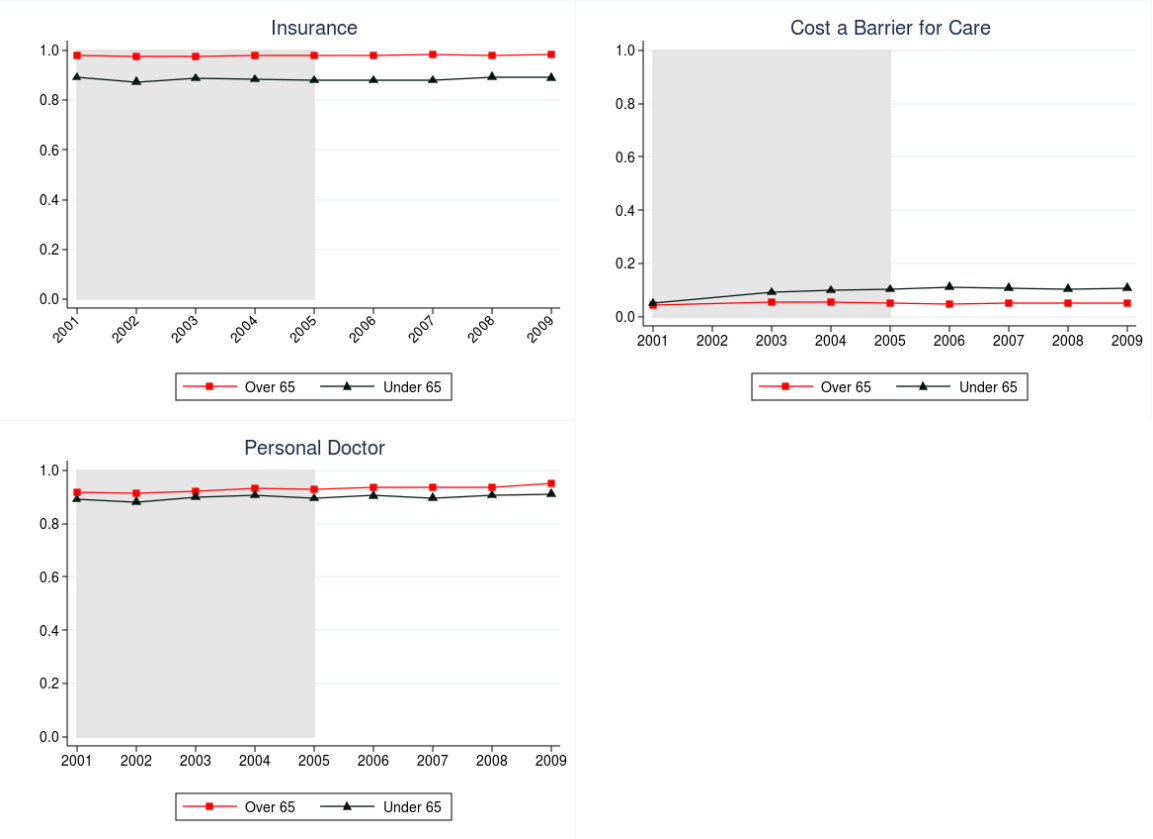


Sugar (gm)



Source: Author's calculations based on NHANES 2000-2012. Figure displays coefficient and 95% confidence intervals for the interaction of treatment and each year indicator. All regressions also control for the treatment group indicator, sex, age, race/ethnicity, educational attainment, marital status, household income, household size, and year-fixed effects. Data is adjusted by NHANES sample weights.

Appendix Figure 3- 2. Trends Graphs for Additional BRFSS Outcomes



Source: Author's calculations based on BRFSS 2001-2009. Sample is restricted to include adults aged 60 to 70; adults aged 65 are excluded from the sample. The treatment group is adults aged 60 to 64; the control group is adults aged 66 to 70.

3-C. Appendix 3 Tables

Appendix Table 3- 2. Demographic Characteristics of the BRFSS Sample

	Treatment Group (Ages 66-70)		Control Group (Ages 60-64)		Pre-Treatment Difference
	Pre-Treatment (2001-05)	Post-Treatment (2006-09)	Pre-Treatment (2001-05)	Post-Treatment (2006-09)	
Age	67.97 (1.424)	67.92 (1.431)	61.86 (1.412)	61.85 (1.421)	6.11***
Female	0.517 (0.500)	0.512 (0.500)	0.502 (0.500)	0.492 (0.500)	0.024***
Married	0.660 (0.474)	0.672 (0.469)	0.699 (0.459)	0.703 (0.457)	-0.038***
Years Schooling	13.12 (2.346)	13.38 (2.289)	13.36 (2.282)	13.70 (2.269)	-0.273***
Unemployed	0.0140 (0.118)	0.0174 (0.131)	0.0341 (0.182)	0.0415 (0.199)	-0.021***
Household Income (Thousands)	42.21 (23.64)	47.75 (24.68)	49.27 (24.88)	54.28 (25.05)	-7.07***
Household Size	2.027 (0.948)	2.042 (0.964)	2.213 (1.102)	2.213 (1.079)	-0.178***
White	0.803 (0.398)	0.783 (0.412)	0.787 (0.409)	0.773 (0.419)	0.017***
Black	0.0848 (0.279)	0.0878 (0.283)	0.0882 (0.284)	0.0878 (0.283)	-0.004*
Hispanic	0.0654 (0.247)	0.0756 (0.264)	0.0717 (0.258)	0.0809 (0.273)	-0.006*
Other Race	0.0471 (0.212)	0.0532 (0.224)	0.0528 (0.224)	0.0582 (0.234)	-0.006**

Source: Author's calculations based on BRFSS 2001-2009. Sample is restricted to include adults aged 60 to 70; adults aged 65 are excluded from the sample. The treatment group is adults aged 60 to 64; the control group is adults aged 66 to 70. Standard deviations are in parentheses. Data is adjusted by BRFSS sample weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3- 3. DD Regression Results for Additional NHANES Outcomes

	Pooled	Unmarried Females	Married Females	Unmarried Males	Married Males
Total monounsaturated fatty acids (gm)	1.682***	0.968	1.655*	5.241***	1.433
	(0.535)	(0.920)	(0.947)	(1.473)	(1.038)
	[μ=24.2]	[μ=21.0]	[μ=21.8]	[μ=25.0]	[μ=28.9]
Total polyunsaturated fatty acids (gm)	-0.199	0.619	-0.532	1.418	-0.732
	(0.378)	(0.723)	(0.680)	(0.943)	(0.695)
	[μ=14.3]	[μ=12.9]	[μ=13.7]	[μ=13.8]	[μ=16.5]
Dietary fiber (gm)	-0.559*	-0.294	-0.546	1.042	-1.092*
	(0.337)	(0.564)	(0.629)	(0.864)	(0.662)
	[μ=15.6]	[μ=13.9]	[μ=14.4]	[μ=14.4]	[μ=18.0]
Vitamin A, RAE (mcg)	8.121	-38.428	-62.326	-23.431	100.205
	(41.680)	(38.967)	(46.594)	(125.970)	(108.553)
	[μ=660.3]	[μ=624.6]	[μ=633.2]	[μ=760.9]	[μ=681.3]
Thiamin (Vitamin B1) (mg)	-0.007	-0.027	-0.031	0.195**	-0.053
	(0.029)	(0.048)	(0.051)	(0.087)	(0.055)
	[μ=1.5]	[μ=1.3]	[μ=1.3]	[μ=1.5]	[μ=1.7]
Riboflavin (Vitamin B2) (mg)	0.035	-0.045	0.005	0.059	0.108
	(0.042)	(0.061)	(0.070)	(0.131)	(0.089)
	[μ=2.0]	[μ=1.7]	[μ=1.8]	[μ=2.2]	[μ=2.2]
Vitamin B12 (mcg)	0.264	-0.035	-0.633	-1.141	1.698
	(0.428)	(0.292)	(0.514)	(1.109)	(1.152)
	[μ=4.9]	[μ=4.0]	[μ=4.5]	[μ=6.7]	[μ=5.6]
Vitamin B6 (mg)	-0.097**	-0.103	-0.063	-0.003	-0.138*
	(0.039)	(0.067)	(0.065)	(0.123)	(0.074)
	[μ=1.7]	[μ=1.5]	[μ=1.5]	[μ=1.9]	[μ=2.0]
Vitamin C (mg)	-2.113	-7.166	-2.446	14.276	-2.409
	(3.052)	(5.365)	(5.705)	(8.707)	(5.569)
	[μ=94.6]	[μ=89.7]	[μ=93.5]	[μ=79.8]	[μ=102.5]
Vitamin E as alpha-tocopherol (mg)	-0.085	-0.088	-0.303	0.421	0.039
	(0.202)	(0.362)	(0.377)	(0.537)	(0.393)
	[μ=6.3]	[μ=5.7]	[μ=6.1]	[μ=6.0]	[μ=7.3]
Vitamin K (mcg)	-17.681***	-28.501**	-9.015	-32.052	-6.936
	(6.844)	(13.242)	(9.086)	(19.697)	(14.974)
	[μ=99.6]	[μ=113.5]	[μ=87.6]	[μ=95.6]	[μ=95.8]
Calcium (mg)	-6.315	-1.026	5.573	34.692	-40.734
	(18.180)	(30.541)	(34.388)	(54.730)	(34.213)
	[μ=741.1]	[μ=664.8]	[μ=680.6]	[μ=765.4]	[μ=835.2]
Iron (mg)	0.005	-0.483	-0.647	1.529	0.300
	(0.299)	(0.478)	(0.555)	(0.946)	(0.582)
	[μ=14.5]	[μ=12.4]	[μ=13.3]	[μ=15.3]	[μ=16.9]

Zinc (mg)	0.258 (0.257) [μ=10.3]	0.315 (0.378) [μ=8.9]	0.263 (0.420) [μ=9.0]	1.058 (0.937) [μ=10.9]	-0.060 (0.528) [μ=12.3]
Sodium (mg)	-7.524 (52.712) [μ=2,835.8]	5.191 (90.758) [μ=2,462.3]	-75.062 (90.019) [μ=2,574.0]	234.142 (155.829) [μ=2,918.4]	3.584 (103.094) [μ=3,336.1]
Alpha-carotene (mcg)	38.949 (41.410) [μ=386.8]	-36.823 (86.453) [μ=443.7]	-45.035 (78.291) [μ=416.1]	327.449*** (87.157) [μ=257.9]	52.272 (80.546) [μ=350.9]
Beta-carotene (mcg)	-341.736** (155.313) [μ=2,373.5]	-439.613 (304.800) [μ=2,663.4]	-568.110* (291.961) [μ=2,412.6]	231.409 (366.917) [μ=1,854.1]	-293.399 (300.199) [μ=2,230.5]
Caffeine (mg)	21.220*** (7.765) [μ=156.4]	2.881 (13.711) [μ=134.0]	9.035 (11.926) [μ=137.6]	36.341* (21.136) [μ=170.7]	44.066*** (16.272) [μ=186.6]
Alcohol (gm)	-0.158 (0.786) [μ=5.3]	0.071 (1.090) [μ=1.7]	-0.812 (1.011) [μ=3.6]	-0.923 (3.545) [μ=13.4]	0.392 (1.456) [μ=7.2]
Observations	28,404	7,009	7,554	5,024	8,817

Source: Author's calculations based on NHANES 2000-2012. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for the treatment group indicator, sex, age, race/ethnicity, educational attainment, marital status, household income, household size, and year-fixed effects. Data is adjusted by NHANES sample weights. Robust standard errors are in parentheses. Pre-2006 mean for treatment group [μ] is in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3- 4. Pre-Trends Tests for BRFSS Diet Outcomes

	Pre-Trends
Fruit Juice	-0.032 (0.049)
Fruit	-0.004 (0.059)
Green Salad	-0.016 (0.035)
Potatoes	0.007 (0.023)
Carrots	-0.025 (0.035)
Other Vegetables	0.035 (0.061)
Observations	227,526

Source: Author's calculations based on BRFSS 2001-2005. Sample is restricted to include adults aged 60 to 70; adults aged 65 are excluded from sample. Each cell displays the estimated coefficient for the interaction term of treatment group and linear time trend. All regressions also control for sex, age, race/ethnicity, education, marital status, unemployment status, household income, household size, state unemployment rate, state-fixed effects, and quarter/year-fixed effects. Data is adjusted by BRFSS sample weights. State-clustered standard errors are in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3- 5. Potential Mechanisms for Diet Outcomes (BRFSS)

	DD Estimate
Access to Care	
Insurance	-0.001 (0.003) [μ=0.880]
Cost Barrier for Care	-0.010*** (0.004) [μ=0.103]
Personal Doctor	0.007* (0.004) [μ=0.896]
Observations	393,299

Source: Author's calculations based on BRFSS 2001-2009. Sample is restricted to include adults aged 60 to 70; adults aged 65 are excluded from sample. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. All regressions also control for sex, age, race/ethnicity, education, marital status, unemployment status, household income, household size, state unemployment rate, state-fixed effects, and quarter/year-fixed effects. Data is adjusted by BRFSS sample weights. State-clustered standard errors are in parentheses. Pre-2006 mean for treatment group [μ] is in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3- 6. Robustness Checks for BRFSS Outcomes

	Ages 57-73	Ages 58-72	Ages 59-71	Ages 60-70	Ages 61-69	Ages 62-68	Ages 63-67
Fruit Juice	-0.290	-0.058	-0.063	-0.284	-0.580	-0.219	-1.072*
	(0.310)	(0.335)	(0.361)	(0.403)	(0.476)	(0.522)	(0.577)
Fruit	-1.673***	-1.360***	-1.152***	-1.186***	-0.750	-1.274**	-0.292
	(0.361)	(0.386)	(0.407)	(0.440)	(0.493)	(0.560)	(0.685)
Green Salad	-0.462**	-0.614***	-0.597**	-0.590**	-0.761**	-0.676*	-0.588
	(0.208)	(0.223)	(0.245)	(0.268)	(0.307)	(0.348)	(0.449)
Potatoes	-0.134	-0.058	-0.017	0.006	-0.120	-0.004	0.068
	(0.123)	(0.131)	(0.143)	(0.157)	(0.173)	(0.195)	(0.239)
Carrots	-0.301	-0.326	-0.277	-0.338	-0.302	-0.288	-0.310
	(0.191)	(0.208)	(0.232)	(0.223)	(0.218)	(0.256)	(0.312)
Other Vegetables	-1.228***	-1.246***	-1.318***	-1.158***	-0.880*	-0.958*	-0.621
	(0.356)	(0.384)	(0.408)	(0.446)	(0.505)	(0.575)	(0.701)
Observations	363,913	318,401	271,336	227,393	177,240	135,673	88,221

Source: Author's calculations based on BRFSS 2001-2009. Sample is restricted to include adults aged 60 to 70; adults aged 65 are excluded from sample. Each cell displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. State-clustered standard errors in parentheses. All regressions also control for sex, age, race/ethnicity, education, marital status, unemployment status, household income, household size, state unemployment rate, state-fixed effects, and quarter/year-fixed effects. Data is adjusted by BRFSS sample weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3- 7. Sensitivity Analyses for BRFSS Outcomes

	No BRFSS Weights	State-specific Time Trend
Fruit Juice	0.015 (0.199)	-0.289 (0.401)
Fruit	-0.710*** (0.238)	-1.168*** (0.440)
Green Salad	-0.213 (0.134)	-0.590** (0.270)
Potatoes	-0.139 (0.093)	0.013 (0.158)
Carrots	-0.196* (0.105)	-0.347 (0.223)
Other Vegetables	-0.609** (0.246)	-1.154*** (0.446)
Observations	227,393	227,393

Source: Author's calculations based on BRFSS 2001-2009. Adults aged 65 are excluded from sample. Each cell in displays the estimated coefficient for the interaction term of treatment group and post-2006 indicator. State-clustered standard errors in parentheses. All regressions also control for sex, age, race/ethnicity, education, marital status, unemployment status, household income, household size, state unemployment rate, state-fixed effects, and quarter/year-fixed effects. Unless otherwise indicated, data is adjusted by BRFSS sample weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendices for Chapter 4

4-A. Review of Literature on Financial Effects of Health Insurance

Appendix Table 4- 1. Studies that Evaluate the Impact of Health Insurance on Consumption and Financial Outcomes

Paper	Outcomes Studied	Data/Methods	Results
Medicare			
Finkelstein & McKnight. (2008). What did Medicare Do? The Initial Impact of Medicare on Mortality and Out of Pocket Medical Spending. *Journal of Public Economics.*	OOP medical spending	Diff-in-diff comparing individuals aged 65-74 with those aged 55-64; Surveys of Health Service Utilization and Expenditures	Medicare decreased OOP medical expenditure risk.
Barcellos & Jacobson. (2015). The Effects of Medicare on Medical Expenditure Risk and Financial Strain. *AEJ: Economic Policy.*	OOP medical spending; Contact with collections; Borrowing to pay medical bills; Problems paying medical bills	Regression discontinuity at age 65; MEPS	Medicare decreased OOP expenditures, difficulty paying bills, and collections agency contact. No impact on amount owed.
Christelis, Georgarakos, & Sanz-de-Galdeano. (2017). The Impact of Health Insurance on Stockholding: A Regression Discontinuity Approach. Working Paper.	Stockholding	Regression discontinuity at age 65; HRS	Medicare increased stockholding for those with college education but no impact for less-educated people.

Angrisani, Atella, & Brunetti. (2018). Public Health Insurance and Household Portfolio Choices: Unravelling Financial "Side Effects" of Medicare. *Journal of Banking & Finance.*	Stockholding	Fixed effects regression where key independent variable is interaction of Medicare eligibility and poor health; HRS	Before Medicare, households in poor health are less likely to hold stocks, but this gap is eliminated by Medicare.
Medicare Part D			
Engelhardt & Gruber. (2011). Medicare Part D and the Financial Protection of the Elderly. *AEJ: Economic Policy.*	Public vs private drug spending; Crowd-out	Diff-in-diff; Instrumental variables; MEPS	75% crowd-out of private prescription drug coverage and expenditures among the elderly. Large reductions in OOP spending on average, but the bulk of these accrue to a small proportion of the elderly (those with highest spending).
Ayyagari & He. (2016). Medicare Part D and Portfolio Choice. *American Economic Review: Papers and Proceedings.*	Share of financial wealth invested in risky assets (stocks, mutual funds, etc)	Diff-in-diff; HRS	The share of financial wealth invested in risky assets increased by 3.2 percentage points, a 7.2 percent increase relative to the pre-2003 mean.
ACA Young Adult Mandate			
Ali et al. (2016). The ACA's Dependent Coverage Expansion and Out-of-Pocket Spending by Young Adults With Behavioral Health Conditions. *Psychiatric Services.*	OOP medical spending	Diff-in-diff comparing those aged 19-25 with those aged 27-29; MEPS	Reduced OOP spending, providing young adults with additional financial protection.
Han. (2016). The Impacts of the Affordable Care Act Dependent Coverage Provision on College Graduates with Student Loan Debt. Working Paper.	Student loan debt and repayment	Diff-in-diff; PSID	Reduced student loan default rate and increased student loan repayment rate.
Blascak & Mikhed. (2018). Did the ACA's Dependent Coverage Mandate Reduce Financial Distress for Young Adults. Working Paper.	Debt; Delinquency; Bankruptcy	Diff-in-diff; Equifax consumer credit panel data	Decreased past due debt, number of delinquencies, and probability of filing for bankruptcy. Strongest effects among individuals living in counties with highest pre-2010 uninsurance.
ACA Medicaid Expansion			

Brevoort, Grodzicki, & Hackmann. (2017). Medicaid and Financial Health. Working Paper.	Unpaid medical bills; Delinquency; Credit scores	Diff-in-diff comparing expansion and non-expansion states; Consumer credit panel data	Expansion reduced households' medical debt, reduced delinquencies, increased credit scores, and improved terms of credit for loans.
Allen et al. (2017). Early Medicaid Expansion Associated with Reduced Payday Borrowing in California. *Health Affairs.*	Payday (high interest) loans	Diff-in-diff comparing California counties that expanded with other counties nationwide; Consumer Financial Services Association	Decreased number of payday loans taken out, number of unique borrowers, and amount of payday debt.
Caswell & Waidmann. (2017). The Affordable Care Act Medicaid Expansions and Personal Finance. *Medical Care Research and Review.*	Bankruptcy; Medical collection balances; Credit scores	Triple difference models exploiting pre-2014 county-level uninsurance rates and Medicaid expansion; Credit bureau data	Improved credit scores, decreased balances past due, decreased probability of high medical collection, and decreased probability of bankruptcy.
Abramowitz. (2018). The Effect of ACA State Medicaid Expansions on Medical Out-of-Pocket Expenditures. *Medical Care Research and Review.*	OOP medical spending	Diff-in-diff comparing expansion and non-expansion states; CPS	Reduced medical OOP expenditures for low-income people.
Gallagher et al. (2018). Medicaid and Household Savings Behavior: New Evidence from Tax Refunds. Working Paper.	Household savings behavior	Instrumental variables exploiting states' Medicaid eligibility rules; Tax data	No change in savings behavior for average low-income household. Medicaid increases refund savings rate for those experiencing financial hardship.
Hu et al. (2018). The Effect of the Affordable Care Act Medicaid Expansion on Financial Wellbeing. *Journal of Public Economics.*	Credit score; Delinquency; Collections; Bankruptcy	Synthetic control and triple difference models; Equifax consumer credit panel data	Reduced number of unpaid bills and amount of debt sent to collections. No change in credit scores and bankruptcy.
Kino, Sato, & Kawachi. (2018). Spillover Benefit of Improved Access to Healthcare on Reducing Worry about Housing and Meal Affordability. *International Journal for Equity in Health.*	Worry or stress about paying rent/mortgage and purchasing food	Instrumental variables method; BRFSS	Reduced probability of being worried and stressed related to purchasing nutritious meals, as well as paying the rent/mortgage.
Miller et al. (2018). The ACA Medicaid Expansion in Michigan and Financial Health. Working Paper.	Collections; Medical collections; Delinquencies; Credit card borrowing; Public records; Bankruptcy	Event study design exploiting variation in enrollment into Healthy Michigan; Transunion consumer credit history and Healthy Michigan administrative data	Reduced unpaid bills, medical bills, over limit credit card spending, delinquencies, and public records (such as evictions).
Levy, Buchmueller, & Nikpay. (2019). The Impact of Medicaid Expansion on Household Consumption. *Eastern Economic Journal.*	Health spending; Non-health spending (food, housing)	Diff-in-diff comparing expansion and non-expansion states; CEX	Small reductions in health spending. No change in non-health consumption.

ACA			
Boudreaux, Gonzales, & Saloner. (2017). Medicaid Financial Burden Declined for Consumers in the Nongroup Market. *Health Affairs.*	OOP medical spending	Descriptive; CPS	Share of nonelderly people in the nongroup insurance market experiencing medical financial burden declined after 2014.
McKenna et al. (2018). The Affordable Care Act Attenuates Financial Strain According to Poverty Level. *Inquiry.*	Health care financial strain (worry about paying medical bills; problems paying medical bills)	Descriptive; NHIS	Decrease in problems paying medical bills, worry about medical bills, not getting medical care due to cost, and delaying medical care due to cost.
Gallagher, Gopalan, & Grinstein-Weiss. (2019). The Effect of Health Insurance on Home Payment Delinquency: Evidence from ACA Marketplace Subsidies. *Journal of Public Economics.*	Home payment delinquencies	Regression discontinuity exploiting income eligibility threshold to receive Marketplace subsidies in non-Medicaid expansion states; Tax data	Subsidized coverage reduced the probability of being delinquent on home payments.
Medicaid			
Leininger, Levy, & Schanzenbach. (2010). Consequences of SCHIP Expansions for Household Well-Being. *Forum for Health Economics & Policy.*	Health spending; Non-health spending	Diff-in-diff approach exploiting variation in SCHIP expansions; CEX	SCHIP decreased medical expenditures and increased overall expenditures (mostly transportation and retirement savings).
Sommers & Oellerich. (2013). The Poverty-Reducing Effect of Medicaid. *Journal of Health Economics.*	Poverty; OOP medical spending	Propensity score method; CPS	Medicaid reduced poverty rate by 0.7 percentage points.
Gross & Notowidigdo. (2011). Health Insurance and the Consumer Bankruptcy Decision: Evidence from Expansions of Medicaid. *Journal of Public Economics.*	Consumer bankruptcy	Exploit variation in Medicaid expansions from 1992 to 2004; pubic database of bankruptcies from Administrative Office of US Courts	Increasing Medicaid eligibility reduced personal bankruptcy but not business bankruptcies.
Dillender. (2017). Medicaid, Family Spending, and the Financial Implications of Crowd-Out. *Journal of Health Economics.*	Family-level medical spending and non-medical spending by category	Instrumental variables strategy to estimate effect of additional family member becoming eligible for Medicaid; CEX	Decreased medical spending and health insurance spending. No change in medical debt or non-health spending.
Oregon Health Insurance Experiment			

Finkelstein et al. (2012). The Oregon Health Insurance Experiment: Evidence from the First Year. *The Quarterly Journal of Economics.*	OOP medical spending; Medical debt; Bankruptcy; Credit score; Overall debt	Randomized controlled trial; Oregon	Medicaid reduced bills sent to collections, medical debt, and financial strain. No effect on bankruptcy, delinquencies, credit scores, and overall debt.
Massachusetts Health Care Reform			
Mazumder & Miller. (2016). The Effects of the Massachusetts Health Reform on Household Financial Distress. *AEJ: Economic Policy.*	Delinquencies; Bankruptcy; Total collections	Exploit variation across counties and age groups using pre-reform insurance coverage; Consumer credit panel data	Reduced delinquencies, personal bankruptcies, and third-party collections. Increased credit scores.
Tennessee Medicaid Disenrollment			
Argys et al. (2017). Losing Public Health Insurance: TennCare Disenrollment and Personal Financial Distress. Working Paper.	Credit scores; Delinquencies; Bankruptcy	Diff-in-diff exploiting variation in pre-reform enrollment across counties; Equifax consumer credit panel data and Tennessee Medicaid administrative data	Disenrollment reduced credit scores. Increased delinquent debt and bankruptcy risk.
Other Settings			
Mahoney (2015). Bankruptcy as Implicit Health Insurance. *American Economic Review.*	OOP medical spending; Insurance coverage	Exploit variation in asset exemption law; MEPS; SIPP; PSID	Uninsured households with a greater financial cost of bankruptcy make higher OOP medical payments. Households with greater wealth at risk are more likely to hold insurance.

Appendices for Chapter 5

5-A. Appendix 5 Tables

Appendix Table 5- 1. Parallel Trends Tests

Total Diagnoses Per 100,000 Population

	(1) All	(2) White	(3) Black	(4) Other	(5) Hispanic
2010 X Expansion	17.91 (22.85)	5.35 (11.04)	82.76 (54.12)	-45.54 (63.35)	29.16 (23.27)
2011 X Expansion	45.69** (22.85)	10.00 (11.04)	58.69 (54.13)	79.42 (63.36)	34.83 (23.28)
2012 X Expansion	1.14 (22.84)	5.45 (11.04)	-0.82 (54.11)	-33.95 (63.34)	33.72 (23.27)
2014 X Expansion	-7.12 (22.84)	23.62** (11.04)	-47.78 (54.10)	-39.87 (63.33)	35.60 (23.26)
2015 X Expansion	18.31 (22.84)	5.84 (11.04)	43.13 (54.10)	-9.55 (63.33)	34.03 (23.26)
N	14652	3666	3660	3660	3666

Early Stage Diagnoses Per 100,000 Population

	(1) All	(2) White	(3) Black	(4) Other	(5) Hispanic
2010 X Expansion	36.54** (17.24)	13.64 (8.36)	60.67 (38.76)	62.26 (51.29)	9.77 (17.08)
2011 X Expansion	38.04** (17.24)	11.28 (8.37)	55.05 (38.76)	71.76 (51.30)	14.25 (17.08)
2012 X Expansion	18.48 (17.24)	6.14 (8.36)	55.52 (38.75)	5.79 (51.28)	6.54 (17.08)
2014 X Expansion	15.99 (17.23)	26.36*** (8.36)	31.01 (38.74)	-11.83 (51.27)	18.54 (17.07)

2015 X	29.40*	9.42	44.56	31.26	32.54*
Expansion	(17.23)	(8.36)	(38.75)	(51.28)	(17.08)
N	14652	3666	3660	3660	3666

Late Stage Diagnoses Per 100,000 Population

	(1) All	(2) White	(3) Black	(4) Other	(5) Hispanic
2010 X	-20.61	-6.18	25.24	-112.17***	10.61
Expansion	(14.06)	(6.55)	(37.52)	(37.59)	(14.19)
2011 X	5.75	0.01	5.31	3.51	14.18
Expansion	(14.07)	(6.55)	(37.53)	(37.60)	(14.19)
2012 X	-15.03	1.98	-40.48	-44.53	22.75
Expansion	(14.06)	(6.55)	(37.51)	(37.58)	(14.19)
2014 X	-23.77*	0.50	-72.88*	-28.45	5.68
Expansion	(14.06)	(6.55)	(37.51)	(37.58)	(14.18)
2015 X	-10.80	-2.08	0.53	-40.97	-0.66
Expansion	(14.06)	(6.55)	(37.51)	(37.58)	(14.19)
N	14652	3666	3660	3660	3666

Unstaged Diagnoses Per 100,000 Population

	(1) All	(2) White	(3) Black	(4) Other	(5) Hispanic
2010 X	1.98	-2.10	-3.15	4.37	8.77
Expansion	(2.65)	(1.98)	(6.61)	(5.83)	(5.48)
2011 X	1.90	-1.28	-1.67	4.14	6.40
Expansion	(2.65)	(1.98)	(6.61)	(5.84)	(5.48)
2012 X	-2.32	-2.67	-15.86**	4.78	4.43
Expansion	(2.65)	(1.98)	(6.61)	(5.83)	(5.48)
2014 X	0.66	-3.24	-5.91	0.41	11.38**
Expansion	(2.65)	(1.98)	(6.61)	(5.83)	(5.48)
2015 X	-0.29	-1.50	-1.96	0.15	2.15
Expansion	(2.65)	(1.98)	(6.61)	(5.83)	(5.48)
N	14652	3666	3660	3660	3666

Source: Authors' calculations based on SEER 2010 to 2015. Sample was restricted to first-time cancer diagnoses for adults aged 19 to 64. N=14,652 county-year observations. All regressions also control for county unemployment rate, whether the county is rural, percent of county that is female, percent that is over age 65, percent in poverty, state fixed effects, and year fixed effects. Standard errors are in parentheses.

* p<0.10, ** p<0.05, *** p<0.01

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RESEARCH AND TEACHING FIELDS

Health economics; Health policy; Applied microeconomics

EMPLOYMENT

2019-	Assistant Professor, Department of Public Administration and Public Policy, American University (Washington, DC) (starting August 2019)
2014-19	Associate Instructor and Research Assistant, Department of Business Economics and Public Policy, Indiana University (Bloomington, IN)
2012-14	Economist, IHS Global Insight (Lexington, MA and Washington, DC)
2011	Research Assistant, Department of Economics, Boston University (Boston, MA)
2010	Analyst, U.S. International Trade Commission (Washington, DC)

EDUCATION

Ph.D. in Business with a concentration in Economics & Public Policy, Indiana University, 2019
Dissertation: Effects of Financial and Non-Financial Incentives on Risky Health Behaviors and Health Outcomes
Committee: Jeffrey Prince, Kosali Simon, Haizhen Lin, Daniel Sacks
M.A. in Economics, Boston University, 2011
B.A. in Economics and Journalism, Boston University, 2011

RESEARCH

Peer-Reviewed Publications

Hollingsworth, Alex, **Aparna Soni**, Aaron Carroll, John Cawley, & Kosali Simon. (2019). "Gains in Health Insurance Coverage Explain Variation in Democratic Vote Share in the 2016 Presidential Election." *PLOS One*. Forthcoming.

Decker, Sandra, Asako Moriya, & **Aparna Soni**. (2018). "Coverage For Self-Employed and Others without Employer Offers Increased After 2014." *Health Affairs*, 37(8), 1238-1242. (Link)

Cawley, John, **Aparna Soni**, & Kosali Simon. (2018). "Third Year of Survey Data Shows Continuing Benefits of Medicaid Expansions for Low-Income Childless Adults in the U.S." *Journal of General Internal Medicine*, 33, 1495-1497. (Link)

Soni, Aparna, Kosali Simon, John Cawley, & Lindsay Sabik. (2018). "Effects of ACA Insurance Expansions on Cancer Diagnoses." *American Journal of Public Health*, 108(2), 216-218. (Link)

Soni, Aparna, Lindsay Sabik, Kosali Simon, & Benjamin Sommers. (2018). "Changes in Insurance Coverage Among Cancer Patients After the Affordable Care Act." *JAMA Oncology*, 4(1), 122-124. (Link)

Soni, Aparna, Marguerite Burns, Laura Dague, & Kosali Simon. (2017). "Medicaid Expansion Status and State Trends in Supplemental Security Income Program Participation." *Health Affairs*, 38(8), 1485-1488. (Link)

Soni, Aparna, Michael Hendryx, & Kosali Simon. (2017). "Medicaid Expansions under the Affordable Care Act and Insurance Coverage in Rural and Urban Areas." *Journal of Rural Health*, 33(2), 217-226. (Link)

Simon, Kosali, **Aparna Soni**, & John Cawley. (2017). "The Impact of Health Insurance on Preventive Care and Health Behaviors: Evidence from the First Two Years of the ACA Medicaid Expansions." *Journal of Policy Analysis and Management*, 36(2), 390-417. (Link)

Papers under Review

Soni, Aparna, Cong Gian, Kosali Simon, & Benjamin Sommers. "Levels of Employment and Community Engagement among Medicaid-Eligible Adults: Implications for Work Requirements."

Woroncowicz, Joanna, **Aparna Soni**, Seth Freedman, & Kosali Simon. "How Have Recent Health Insurance Expansions Affected Coverage Among Artist Occupations?"

Works in Progress

Soni, Aparna. "Health Insurance, Price Changes, and the Demand for Pain Relief Drugs: Evidence from Medicare Part D." (Link)

Soni, Aparna. "Increasing Pharmaceutical Access for the Elderly Improves Functional Outcomes: Implications for Caregivers."

Soni, Aparna. "Consumption Effects of Health Insurance Expansions for Young Adults: Evidence from Scanner Data."

Soni, Aparna. "Drugs and Diet: Ex-Ante Moral Hazard Effects of Prescription Drug Insurance."

Lin, Haizhen, **Aparna Soni**, & Yaying Zhou. "The Impact of Market Concentration on Premiums in the Employer Sponsored Insurance Market."

Soni, Aparna, Seth Freedman, Alex Hollingsworth, & Kosali Simon. "The Impacts of Prescription Drug Monitoring Programs on Opioid Hospitalizations."

Conference Presentations

2019 American Economic Association; American Society of Health Economists (forthcoming)

2018 American Society of Health Economists; Association for Public Policy Analysis and Management; Southern Economic Association

2017 AcademyHealth Annual Research Meeting; AcademyHealth Health Economics Interest Group (poster); Association for Public Policy Analysis and Management; Eastern Economic Association; International Health Economics Association; IU/UofL/VU Health Economics and Policy Workshop; Population Health Science Research Workshop (poster)

2016 American Society of Health Economists (poster); IU/UofL/VU Health Economics and Policy Workshop

Invited Seminars

2018 American University; Boston University; George Washington University; Indiana University-Purdue University Indianapolis; Pennsylvania State University; US Food and Drug Administration; University of Utah

Media Coverage of Research

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EXTERNAL RESEARCH SUPPORT

2018 Research Grant, Horowitz Foundation for Social Policy. ($7,500)
"Reducing Health Disparities Among People Diagnosed with Cancer: The Role of Public Health Insurance Expansions"

INTERNAL RESEARCH SUPPORT

2018 Doctoral Student Research Productivity Award, Indiana University ($3,700)
2017 Doctoral Student Research Productivity Award, Indiana University ($3,700)
2017 Doctoral Research Travel Grant, Indiana University ($600)
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2016 Travel Grant, IU Graduate and Professional Student Government ($500)

HONORS AND AWARDS

2018 Associate Instructor Teaching Award, Indiana University
2018 Panschar Teaching Award Finalist, Indiana University
2017 Best Poster Award, AcademyHealth – Health Economics Interest Group
2017 Panschar Teaching Award Finalist, Indiana University
2016 Clements & Clements Scholarship Essay Contest Winner
2007-11 Legacy Scholarship, Boston University ($4,000)
2007-11 Trustee Scholarship, Boston University ($148,694)

TEACHING

Instructor of Record

2015-18 G300: Intro to Managerial Economics; Indiana University (4 classes)
2018 G202: Business, Government, and Society; Indiana University (1 class)

Teaching Assistant

2016 G350: Business Econometrics; Indiana University (1 term)
2016-18 G202: Business, Government, and Society; Indiana University (5 terms)

Guest Lectures

2018 P303: Health Psychology; Indiana University ("Health Care Reform and the Affordable Care Act")

SERVICE TO UNIVERSITY

2017-18 Mentor for Indiana University summer health research scholars program
2017 University representative for "PhD Project" conference to support underrepresented minorities in pursuing graduate degrees
2016-17 Coordinator for weekly Indiana University health policy workshop

SERVICE TO PROFESSION

Conference Discussant

2019 American Society of Health Economists (forthcoming)
2018 Association for Public Policy Analysis and Management; Southern Economic Association
2017 Association for Public Policy Analysis and Management; International Health Economics Association; Eastern Economic Association

Journal Reviewing

American Journal of Preventive Medicine (1 manuscript); Annals of Internal Medicine (3); Health Affairs (3); Health Economics (3); Health Services Research (1); Journal of Health Economics (3); Journal of Rural Health (3); Obesity (1); Preventive Medicine (1); World Medical and Health Policy (1)

Conference Abstract Reviewing

American Society of Health Economists; AcademyHealth Annual Research Meeting

Professional Affiliations

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